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THE PSYCHOLOGICAL REVIEW

PERSISTENT PROBLEMS IN SYSTEMATIC PSYCHOLOGY V. ATTENTION AND ASSOCIATION

BY RAYMOND HOLDER WHEELER

University of Kansas

The series of papers of which this is the fifth deals with a group of problems in systematic psychology which have always given trouble, even to the extent in most instances of causing extended controversies (1). Just at this time when principles of psychology are undergoing perhaps the most profound reformulation of any period in the history of the science it is certainly not amiss to inspect critically several issues which seem to be neglected or lightly cast aside. While atomism and introspection have been the targets for the most vigorous of recent attacks upon conventional psychology, attention and association have suffered from a less direct assault. In our opinion, however, they owe their present status to the bad company they were keeping and not to weaknesses inherent in the logic upon which they were based.

Accordingly, the specific purpose of this paper is to discuss the problem: Are attention and association legitimate and useful concepts in present-day psychology? In other words, are there issues current today which logically call for either or both of these terms?

We cannot trace the history of attention and association in detail. However, a general survey is germane to our problem. When Aristotle summarized certain uniformities in thinking in his famous laws of association he was not concerned with a systematic problem in psychology but rather with a formula for the mechanics of logical discourse. When

the Neo-Platonists asked themselves if it were possible to have an idea and not know it, they were involved in an epistemological problem. But psychology was destined to evolve as a science of consciousness, *i.e.* of the process of knowing, and as such, inherited two fundamental concepts both of which were to be seriously questioned when epistemological problems were separated from the psychological. These two concepts were (1) association and (2) levels or degrees of consciousness. The former turned out to be a British product and the latter a continental product. The one was a direct outgrowth of empiricism and the inductive method, the other a product of rationalism and deduction. Both concepts played practically the same rôle in the history of psychology—occupied much the same logical place in their respective systems. As a consequence, we find that today there is much that is superfluous in our psychological thinking because of the fact that we have been using uncritically the products of two opposed sets of assumptions—of two opposed epistemological theories—the one seeking the source of the knowing process from within, the other seeking it from without. In short attention and association have been used from different standpoints to account largely for the same set of facts. In the abstract this set of facts is the orderly grouping and seriation of detail in mental life.

The far-reaching assumptions of Locke (2) and profound empirical analyses of Berkeley (3) paved the way for a structuralism for which a supplementary and dynamic principle was sought in association. Mind was at the outset a *tabula rasa*. There were no innate ideas. Little by little the structure of mind was built up by a process of summation or accretion on the basis exclusively of sense impressions. Gradually it became evident that ideas were analysable into images which latter were replicas of sensation. But these discrete items of experience were found to be grouped into complexes and seriations in an orderly fashion. What was the mechanism which accounted for this state of affairs? What was the common denominator? Feeling and will were supposedly experiences which, like perceptions and ideas,

followed the law of association and were not, therefore, explanatory factors. How then was the unity of mind explained?

The contents or structures of consciousness had been made absolute and inert entities. They could not function as parts of a whole unless there were something that brought them together. Association was the possible tool. It was already on the ground. It became an active, dynamic and formative principle, a subconscious process with Hamilton (**4**), a process akin to chemical union or affinity in Mill (**5**), a revival process in Spencer (**6**), and a principle of acquisition in Lewes (**7**). Association as a process and not merely the name for organization and unity in a mental life conditioned by contiguity of stimulation of two or more experiences, carried the systematic psychology of the later associationists over to the attention psychology of the continental group. Association became practically an apperception-process with the course of experience at any one time directed by events of the past.

Possibly this later tendency to conceive of a dynamic principle was somewhat half-hearted in comparison with the procedure of the continentals but it can hardly be affirmed that associationism declined because it failed to recognize the necessity of functional concepts. Nor is it altogether safe to blame its downfall to the fact that it confused mental contents with logical units—meanings. Meanings are as changing and flexible as any other detail or aspect of experience, clearly as flexible units to handle as sensations and images.

Indeed, recent conventional psychology has been in some respects as associationistic as Hartley (**8**). The particular epistemology of the earlier associationists may have been discarded; Spencer's interest in evolution, Lewes' interest in social behavior, Bain's (**9**) motor psychology and Ward's self and interest psychology (**10**) may have changed the emphasis and divided our attention; but associationism lingered on. Today it is as much an issue as ever. Current behaviorism with its emphasis upon the conditioned reflex harbors the same logic. We have innate tendencies instead of perceptions,

habits instead of ideas, and the law of contiguity is as triumphant as ever! The attempt to derive unity from nonunity is as futile in behaviorism as in Hartley's associationism.

To be sure, associationism made mind passive, at first. Man was a victim of external stimulation, a product of environment. Initiative and invention were out of the question. Bain's emphasis on motor activity, pleasure and pain, helped little. If an experience happened to be emotionally tinged the individual was lucky. He then had will and could learn. Our conventional educational psychology is essentially associationistic in this manner. Consider the law of exercise (11). A certain performance is made easier by repetition. We go back to the logic of sense impression with subsequent revival, right where associationism started. If the individual is lucky enough to have his activities restimulated his habits will become fixed. Of course there is an internal urge to fall back on—instinct, which keeps the individual trying; but when this fails he may be lucky again. He may have succeeded accidentally; his experiences may have been pleasant and therefore fixed more readily. If unpleasant they will have been inhibited. The basic notion of contiguity is implied throughout. Bonds are exact logical duplicates of Hamilton's subconscious. Integration is merely speeded or checked by supplementary factors which are as much secondary laws of association as those of Brown (12). Innate tendencies are just so many *tabula rasas* waiting to be supplied with content.

Meanwhile we have been busy with principles handed down from Leibnitz (13), Herbart (14) and Wundt (15) and with a rationalism as far-reaching as British empiricism. Part of consciousness was subconscious. Mind was not a *tabula rasa.* One had innate ideas. A given experience was an interaction between the conscious and the subconscious mental levels. Knowledge did not come exclusively from the senses. Sense impressions became conscious if there were the proper articulation between them and the subconscious mind. This articulation was an assimilation process. The subconscious directed it. Mind possessed detail and derived its

dynamic character from the active nature of each content, whether it were an idea made definite and concrete through experience or whether it were an innate idea or wish.

This interaction came slowly to be regarded as a process of clarification. Experiences were made clear, *i.e.*, came into existence by a sort of fitting process. This was Herbart's apperception. Then Wundt discarded the two-level notion of the conscious and subconscious and substituted for it the notion of focus and margin (**16**). Experience was still to be accounted for in terms of apperception—a clarification process phenomenologically observable (*?*) in the transition of a mental process from fringe to focus and physiologically traceable in theory to the functioning of the frontal lobes (**17**). In substituting the terms focus and margin for conscious and subconscious our modern conception of attention, as clearness, was born. Titchener at first dropped from this notion Wundt's emphasis upon attention (**18**) as a process and made attention an attribute of sensation, but later returned somewhat closer to the Wundtian position in making it a dimension of consciousness—attensity (**19**).

Attention psychology in many ways was an exact counterpart of association psychology. In terms of an interaction between levels those who accepted rationalistic doctrines attempted to account for law and order in mental life. They chose the notion of the subconscious or its equivalent in the light of their rationalistic philosophy just as the British chose the law of contiguity in the light of their empirical philosophy. Both schools faced the same logical problems. Each allowed its epistemological interests to dictate its psychological system-making. Such was the force of their philosophical heritage and bent.

Now we come to our main thesis. Abstract both from association and attention psychology the epistemological assumptions which colored their theories of integration and unity in mental life, we find issues which are as alive today as at the time they were first confronted. Not only this, but if systematic psychology is to be built upon the remains it will not do to thrust either the problem of association or attention

lightly aside. The logic which compelled the British associationists and the continental apperceptionists to search for a common denominator in terms of which to envisage continuity and unity in complex mental life is as vital and legitimate today as it was then. Once we admit complexity—concede detail—we assume a function or functions for which experimental interests and the conception of problems demand a name. Consistent progress will not be made in the absence of adequate systematizing hypotheses.

Association psychology as a system in which the complex is explained in terms of the simple is receiving its real death blow today. As a system which emphasized structural analysis, *and at the same time* sought for a single condition under which these abstracted quantities operated, it is likewise receiving its death blow today. That single condition was contiguity of impression and in current behaviorism it is contiguity of stimulation (19a). The issue today has been shorn, particularly by the efforts of the configurationists, of its epistemological aspects and of the false logic of trying to obtain unity from a situation which lacks it. But we still have detail to contend with. As long as there are details there is always the problem of relationship between details.

Attention psychology had corresponding faults. The distinction between the subconscious and conscious was too arbitrary, inconsistent, and led to infinite regresses. The subconscious mind was by definition something unobservable and the hypothesis, therefore, was unverifiable. The distinction between focus and margin was equally as unwieldy and self contradictory when interpreted functionally. To observe fringe meant to make it the focus. In other words fringe was unobservable. It looked for a time as if attention psychology was saved by a distinction between process and meaning—attributive and cognitive clearness—but this distinction seems untenable (20). The fact of clearness seems rather to be a perception, in itself complex. Moreover the process by which clearness is perceived is always analytical, as is any direct observation, which means that the observed phenomenon is complex. If this be true, attention defined as clearness becomes meaningless.

The forerunner of attention, apperception, was shorn of its entanglements with a subconscious mind when the emphasis was laid upon such concepts as *Aufgabe*, mental set, habit and the like as 'conditions of attention.' Here we come to the exact relationship between attention and association. Attention stood for the process of selection going on in the 'stream of consciousness,' *i.e.*, for continuous changes of content questionably defined as changes, ultimately, in clearness relations. But selection did not tell the whole story. Certain conditions of attention were more powerful than others. The selective process took place in orderly fashion. The supplementary concept needed in order to complete one's understanding of the situation was already in existence and it was taken over. It was the concept of association. Here was something which apparently explained why at a given time the selective process went in a particular direction rather than another. Association was the 'why' of attention. Conversely it became apparent that association did not answer the question 'how' *i.e.*, it did not logically and theoretically furnish the *modus operandi*. It must be regarded as a process, an activity. Thus defined it became to that extent synonymous with apperception. Accordingly, attention became the 'how' of association. The two principles thus crossed each other's paths! All of this was inevitable as long as the data of psychology were defined as mental processes. The facts of change, continuity, unity, demanded classification under a general head of some sort and the term of necessity was subjective in connotation. The psychologist was as unable to get along, logically, without such concepts as attention and association, as was the physicist without reference to energy or an equivalent concept.

II

Now that subjectivism and its parent, dualism, have been superceded in psychological theory by viewpoints which disregard the conscious aspect of behavior, as such, it is to be expected that principles definitely tied up with psychology as the science of consciousness should be regarded with suspicion.

The behaviorists have been denying association explicitly but not implicitly. The configurational writers like the introspectionists have abandoned association as an explanatory principle (21). However there seems distinctly to be a difference in the degree to which the two latter groups seriously accept association from a systematic viewpoint. For example, in Titchener's context theory of meaning (22) association plays a prominent rôle, not as an explanatory principle but as a descriptive term depicting context, organization, unity. Sensory and imaginal processes, we are told, derive context from 'situations,' and 'situations' are (1) stimuli which the organism selects, unifies and focalizes and (2) meaningful experiences of conscious presents, *i.e.*, the unified, organized mental processes in terms of which the organism is aware of the stimulus situation or responds to it (23). Then later we learn, "There are as many modes or forms of association as there are ways of being together within a conscious present (24)." The fact of being together demands a descriptive term and that term is association. The whole is not a whole without reference to its parts and its parts are integrated, balanced, tied up dynamically somehow in a pattern, else the parts do not constitute the whole at all. Association stands for this fact of integration. It is not a cause but a condition of being, depicting relationships between parts. Thus Titchener recognized the problems of whole, unity, *et cetera*, and evidently saw clearly that the concept of association was not to be abandoned. Indeed he spoke of association as a mechanism (25) but we are to use such a concept with extreme caution (26).

On the other hand the configurationists retain association as a fact but seemingly neglect it, or reveal prejudice against it as a problem. There are evidently several reasons for this, among which are the following. First, they would consider parts only in their relation to the wholes, not in their relation to each other. Parts are not regarded systematically as existing in relation to other parts. This would naturally follow from their vigorous attempt to avoid atomism. Second, association evolved in a psychological system which abstracted

consciousness from the activities of the organism as a whole, *i.e.*, the system was dualistic. In a monistic view, like the configurational, association would on first thought appear superfluous, if not entirely out of place. Third, the shift from direct to functional analysis quite naturally turns one's attention away from the thing he is dealing with to its cause. Study a phenomenon of behavior as it is affected by external changes and explain it in terms of those changes. So again, internal organization as such is systematically neglected.

Let us inspect more closely these different attitudes taken toward association by the configurationists and introspectionists.

(1) We are constantly hearing about 'parts,' 'members,' 'factors' in a configuration, 'membership character' and the like in the *Gestalt* literature. Descriptive units (not elements) are evidently necessary. To substitute 'member' for sensation does not conceal the fact that we have details integrated into complexes, patterns or wholes. Are we to envisage these details as having no relation to one another and as dependent exclusively for their existence upon the whole of which they are the members or parts? Are we to assume that the members depend for their existence upon the 'whole' character of the stimulus pattern excluding any efficacy whatever of the detailed features of the stimulus? Obviously not. This gives no value at all to membership. Parts are superfluous. If parts are superfluous so are wholes, for the latter have no meaning without reference to parts. In short 'membership' introduces the problem of relationship between members together with the problem of relationship of these members to details of the stimulus pattern. We thus return to the temporal and spatial relations of stimulus details with which the old law of association was concerned. While the problem need not be handled as the associationists handled it, atomistically, as long as configurations do change little by little, as long as we have to deal with phenomena of differentiation and growth, integration and disintegration, with definite changes in environment producing definite modifications of behavior, there still remains the problem of association, or interrelationship of structures.

Indeed the physicists have their notions of gravity, attraction and repulsion or their equivalent, dealing with interrelationships of structures all the way from electrons to heavenly bodies; they also have such concepts as surface tension, capillary attraction *et cetera*, all of which are attempts to comprehend relationships between structures. Such postulates are needed to complete the conceptual system, to describe effectively physical situations, to name everything that is going on. Psychology has always had similar types of concepts. The postulate of conation and a whole array of so-called relational experiences are well known examples. The least animistic of such conceptions in psychology seems to be that of association. Whatever the outcome may be for psychology, the logic of organizing our own facts and postulates is not different from that of physics!

(2) A monistic system does not eliminate the problem of structure-function. On the structural side we have a phenomenology which seems to be giving the configurationists some systematic trouble, due we believe to their prejudice against 'analysis.' They are not abandoning phenomenology, for we still hear about color, brightness, tone, movements of various kinds all to be described differently, and various other phenomena which were once called sensory processes and images but which in the newer psychology have no name at all! We still read about laws of contrast, depth perception, localization, feeling and the higher thought processes. In all of this the phenomenology of conventional psychology is taken for granted.

The struggles of configurationism with this phenomenology show clearly in the vagueness and scarcity of all of their descriptive terms (not taken over directly from conventional psychology), even the term *Gestalt*. Our concepts of sensory process, perception, feeling, thought—indeed all psychological phenomena—have evolved upon the basic framework of association and attention; they have meaning only with reference to these concepts. A complete denial of association (and attention) would mean a complete negation of phenomenology. Throw out the foundation, and the super-

structure must fall. However, the new psychology has not fallen and, in our estimation, its strength lies as much in its heritage as in its new discoveries! Our eyes are at present turned to the new features of configurationism; but it contains many old ones, one of which is the implication, at least, of association as a necessary systematic concept. If we are to take a phenomenology for granted we should also seriously recognize description. Description is useless in the absence of classification. Configurations have neither been classified nor systematically described. The older psychology faced the issue fairly and squarely. Details were recognized; pattern formations were known; each member within the pattern had its logical relations with its partners. *Association and attention were descriptive of integration at the conscious level.*

Problems pertaining to the emergence of quality or figure from ground, mental growth, increasing or decreasing complexity of any pattern reaction, become unmanageable in the absence of description. Not to account for mental growth are attention and association necessary, but to complete the description and to summarize facts on the structural side. To be known, the facts of function must be stated and described, which means nothing more than to give them structure; they are not named, recognized and studied without a phenomenological technique and vocabulary.

(3) Functional analysis as such merely takes for granted structure and description. Structural analysis as a problem will be in ill favor as long as the interests and demands of current investigators are exclusively explanation and control. This is not a serious situation until the functionalist forgets how much he takes for granted by way of structure and enters blind alleys for want of a balanced scientific purpose. Not many years ago it was said of Professor Titchener that he believed there was sufficient functional psychology of a common sense character already known to keep the structuralist busily engaged in scientific research for an indefinite period of time. He regarded functional psychology as unscientific and structural analysis as the only really scientific

method. In the short period of 15 or 20 years—extraordinarily brief as periods go in the history of psychology—the configurationists and their followers seem to be taking the opposite stand and one which, of course, is equally as extreme, namely, that common sense has already supplied us with enough structural analysis to maintain indefinitely a program of functional analysis. The tide has turned. Structural analysis is not scientific. One cannot help but be suspicious of such extreme views and still more suspicious of so rapid a shift from one to the other.

But there is a more weighty consideration. By what process of reasoning does a scientist take for granted structure as a necessary thing to begin with—accept it as something to start with—and yet oppose the procedure by means of which this preliminary and necessary knowledge of structure was originally obtained and is later refined! We can heartily agree with the functionalist who insists that structural information is necessary to start with, but we must insist that adequate knowledge of function attends *adequate* knowledge of structure. Problems in physiology take for granted knowledge of the organism as structure and these problems can be refined in all directions only by refining our histological knowledge. In other words, structural analysis is not a method to be employed, when a science is young, as a makeshift until a functional analysis is invented; it is a method to be carried on indefinitely and one which, as much as anything else, determines the advances made in prediction and control. It is the method of description by direct observation and analysis. Would we be studying nerve conduction if we did not know about the neuron? Do problems in digestion have anything whatever to do with histological knowledge about the linings of the stomach? Do problems connected with the absorption of oxygen and elimination of carbon dioxide through the lungs have anything whatever to do with knowledge of capillaries in the lungs? Can we understand growth without reference to cells? Is it at all important in the study of the learning process to know the observer's attitude, intent, mood, and how these factors effect learning? If these

are influencing factors should we not know as much about them as possible? And does knowledge about them not involve description and classification? To assume structural knowledge obtained by analysis, as preliminary in science, is an acceptance in principle, of the validity of structural analysis. Curious that it should be valid to begin with but when subjected to control in the laboratory be in principle invalid (**27**)! Of course it should be understood that we are quite in agreement with the configurationists in their insistence that the systematic use often made of descriptive data is pointless, and that the introspectionist has been guilty of mistakes. But we have yet to be convinced that the datum, as such, or that the procedure, as such, is not equal in importance, to functional data and functional analysis.

Turning now to the present status of attention, there are plenty of evidences that it is still a live issue, despite counter affirmations (**28**). Take for example the concepts of figure and ground, two so-called properties of structure. How closely in their use of these terms do the *Gestalt* psychologists come to the conventional use of focus and background of consciousness. True that attention was regarded as a property of consciousness while figure and ground are properties of structure, but the shift in emphasis is not crucial. We still face the same problem, essentially, of balance of detail. Figure and ground 'behave' as did focus and margin. We have the familiar phenomena of range, size, definiteness of detail, fluctuation, interest, *Aufgabe*, mental set, *Einstellung* and the like. Very likely attention is as badly in need of a redefinition as association; moreover it should be a descriptive rather than an explanatory concept, pointing to phenomenological balance between the members of any configuration, and to changes in phenomenological properties. Thus while association describes the relationship between parts, attention describes the relation of parts to the whole.

Thus we see that the facts of unity and wholeness so greatly stressed by the configurationists suffer, in the newer psychology, for the want of detailed systematic treatment. There is unity in phenomena everywhere, presumably, and

that unity is given a specific name and its properties specified wherever possible, at least for practical purposes. The atom is still an atom in spite of the theory that it is a 'solar system.' The term atom signifies the unity of the system and the existence of properties which electrons and protons alone do not possess. It is the same with the molecule in relation to the atom, a bodily organ with reference to its cells, man with reference to his muscles, bone and organs, mind with reference to nerve and muscle. Association and attention depict those unique characteristics of unity in behavior and mental life when terms like integration, interaction, pattern reaction and the like are too general. We speak of association and attention in connection with behavior for the same reason that we speak of man in connection with a certain anatomical structure. It does not do the descriptive facts justice to call man an aggregation of protons and electrons, so we call him a person, a man. It does not do conscious behavior justice to describe it on the physical or physiological level (29). But even should one insist upon a strictly 'behavioristic' system he would still be confronted with an attention phenomenon in the reaction which momentarily claims the use of the final common path.

That is, if we rely exclusively upon terms pertaining to phenomena of simpler character as are studied by the physicist we shall not obtain a true picture of the psychological datum; it will not be discovered and understood in its true relationships. While the concepts of attention and association may have been wrongly applied at times and overdone, they did satisfy the logical demand for a common denominator—for general, unifying concepts—a demand which the newer psychology neglects, even in its emphasis upon unity and wholes.

Perhaps a return to monistic conceptions of mind and body have had something to do with this assumption that attention and association may be abandoned. A principle of physical science—the general concept of energy—suffices, it is said. Yes, but energy is described in its numerous *forms* in physics, each form exhibiting different characteristics, also having a

name describing it! We should do the same thing in psychology, describe that energy as it is observed. Observed as human behavior it exhibits numerous forms and characteristics. Attention and association may be 'physical'! But what of it? They are still attention and association.

And finally, the objection of the new functionalism to attention and association is consistent with its attempt to subsume both the structural and functional aspects of the psychological datum under the same conceptual system. Under such a viewpoint one can naturally think of but one type of thing at a time, either structure or function, but not both. One apparently assumes that because descriptive and interpretive problems can be referred to the same category of concepts, structure takes care of itself systematically if function is properly treated. Actually neither the structural nor the functional problem is adequately handled in this view.

The reader may object to our dichotomy between structure and function (30) on the ground of its arbitrariness and lack of observational confirmation. In nature, it is said, we do not find structure and function separated. They are one. The writer would go even farther and insist that in nature as it is observed there is no unity of structure and function, for there neither, as such, exists. Concrete events are the things observed. To speak of them either as structures or functions is interpretation. If we wish to approach analytically the problem of what is observed we at once find an additional term necessary, such as content or structure, or unit, or part, or what not. The thinking or observational process demands it. From that standpoint the observed datum is always a structure, the structural feature of the situation being a contribution of the observer. Functional features are interpretations by the observer of the relationships between these structures. But enumerating, describing, classifying, specifying, all end in structure, as do prediction and control which are temporal specifications.

No, our dichotomy is not found in the object or datum; it is not in the nature of the thing observed. It is in the nature of the observer! Accordingly it crops out inevitably during

the course of observation and interpretation of nature. It is a technique of the knowing process. As such it is an epistemological fact the ignorance of which has led science into innumerable difficulties, has led us into the most curious paradoxes, and has caused numerous controversies which need never have arisen (31).

Now to summarize briefly. With subjectivism abandoned, together with empirical and rationalistic attempts to account for ideas, psychological theory has become more neutral with respect to metaphysical and epistemological problems. Under these conditions do we still need the concepts of association and attention—two outgrowths of metaphysical and epistemological psychology? As originally used the term association meant not only organization of mental processes but the reason for that organization; it was a self-sufficient principle—both cause and effect. While we no longer regard it as cause we still need the concept to mean integration or organization on the level of conscious behavior, first because a string of 'lower-level' (32) words is meaningless and secondly because *unity is phenomenal and like any phenomenon—demands description.* It is the problem of pattern, as such. Neither did empiricism or subjectivism hit upon the essential feature of association. Even bond psychology failed in this respect. The lasting issue is, are we to neglect organization as such, consider it to have no problems in its own right, to manifest no characteristics of its own—in other words not to exist? And the newer psychology which does stress this unity curiously neglects the one concept in terms of which to envisage that unity at the level of conscious behavior, namely association.

With respect to attention, the critical issue is not whether to find in it a dynamic principle of selection but whether the fact of selection at the level of conscious behavior is to be recognized as a problem in itself, and, being recognized, described. In other words, are we to recognize events whose structural or phenomenological evidences are change and variability in the details of mental life—*i.e.*, the facts of phenomenological balance? It is not the issue of apperception, or subconscious, whether there can be a focus or margin, but

the issue of pattern forms and pattern limits. Association, then, is the problem of pattern. Attention is the problem of the form and limits of pattern (**33**).

REFERENCES

1. 'Persistent Problems in Systematic Psychology I. A Philosophical Heritage, PSYCHOL. REV., 1925, **32**, 179–191; II. 'The Psychological Datum,' PSYCHOL' REV., 1925, **32**, 251–265; III. 'The Stimulus-Error and Complete Introspection,' PSYCHOL. REV., 1925, **32**, 443–456; IV. 'Structural versus Functional Analysis,' (To appear in) *J. Gen. Psychol.*, 1928, **1**, 91–107.
2. 'Essay Concerning Human Understanding,' 1690.
3. 'Essay Toward a New Theory of Vision,' 1709.
4. 'Lectures on Metaphysics,' 1859. *Cf.* KLEMM, O., 'History of Psychology,' 1914, 173 *f.*
5. MILL, J. S., 'Logic,' 1843. *Cf.* WARREN, H. C., 'History of Association Psychology,' 1921, 98 *f.*
6. 'Principles of Psychology,' 3d ed., 1880. *Cf.* WARREN, *op. cit.*, 121 *ff.*
7. 'Problems of Life and Mind,' 1874–9. *Cf.* WARREN, *op. cit.*, 137 *ff.*
8. 'Observations on Man etc.' 1749. *Cf.* WARREN, *op. cit.*, 50 *ff.*
9. 'Emotions and Will,' 1859.
10. 'Psychology' in *Ency. Brit.*, 10th ed., 1902; 'Psychological Principles,' 1918.
11. THORNDIKE, E. L., 'Educational Psychology, I. The Original Nature of Man,' 1913.
12. BROWN, T., 'Lectures on the Philosophy of the Human Mind,' 1820. *Cf.* WARREN, *op. cit.*, 71 *ff.*
13. 'Meditations,' 1684. *Cf.* KLEMM, *op. cit.*
14. 'Psychologie als Wissenschaft,' 1824–5.
15. 'Grundzüge der physiologischen Psychologie,' 6th ed., 1911.
16. WUNDT, W., *op. cit.*, Vol. 3, 306 *ff.*
17. IDEM, Vol. 1, 378 *ff.*
18. 'Textbook of Psychology,' 1913.
19. Note in *Amer. J. Psychol.*, 1924, **35**, 156.

19*a*. From which we also obtain our notion of trial and error learning—the notion that complex behavior is built upon reflex action.

20. WHEELER, R. H., 'Some Problems of Meaning,' *Amer. J. Psychol.*, 1923, **34**, 185–202, especially 196 *f.* 'Persistent Problems, III.,' PSYCHOL. REV., 1925, **32**, 451 *ff.*
21. LEWIN, K., 'Das Problem der Willensmessung und das Grundgesetz der Assoziation,' *Psychol. Forsch.*, 1922, **1**, 191–302; 1922, **2**, 65–141.
    KOFFKA, K., 'Growth of the Mind,' 1925, 245 *f*; 'Perception, an Introduction to the Gestalt Theorie,' PSYCHOL. BULL., 1922, **19**, 534 *f.*
    TITCHENER, E. B., 'Textbook of Psychology,' 1913, 357 *ff*, 374 *ff*, 389.
22. TITCHENER, E. B., *op. cit.*, 367 *ff.*
23. *Op. cit.*, 369.
24. *Op. cit.*, 389.
25. *Op. cit.*, 386, 388.
26. *Op. cit.*, 395.
27. *Cf.* Harry Helson's discussion of the configurationist position. 'The Psychology of Gestalt,' *Amer. J. Psychol.*, 1925, **36**, 345; also his excellent discussion of the problem from his own standpoint, *loc. cit.*, 1926, **37**, 192 *ff.*

28. KOFFKA, K., PSYCHOL. BULL., 1922, 19, 535; Köhler, W., 'Komplextheorie und Gestalt-theorie,' *Psychol. Forsch.*, 1925, 6, 358.
29. WHEELER, R. H., 'Outline of a System of Psychology,' PSYCHOL. REV., 1923, 30, 151–163.
30. WHEELER, R. H., 'The Static and Dynamic in the Logic of Science,' *Monist*, 1923, 33, 556–567.
31. Creation idea of causation; Zeno's paradox of the arrow and target; 'bead-hypothesis' of continuity of motion; the atom as a variable or compressible thing versus a fixed and incompressible thing; 'mind-stuff' theory; mind-body problem; act versus content; imageless thought controversy; process versus meaning; stimulus-error and a host of others.
32. Compare "I was not paying attention to what you were saying" with "My neurons and muscle cells were not integrating and coöperating properly with respect to the auditory vibrations which emanated from your throat and impinged upon my organs of Corti."
33. *Cf.* STOUT, G. F., 'Analytic Psychology,' 1913, 2, Ch. 3, where he discusses form of combination and formula of combination.

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FACTORS INFLUENCING THE RELATIVE ECONOMY OF MASSED AND DISTRIBUTED PRACTICE IN LEARNING

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The purpose of this paper is to review the literature on the optimal distribution of work and rest periods in view of determining how and to what extent the various conditions of learning affect the relative economy of different degrees of distribution. The various factors which have been shown in the literature to affect the margin of superiority of spaced over unspaced learning will be enumerated, and a rough approximation of their individual importances and inter-relations will be attempted. The following factors have received experimental consideration and are of major importance: first, the general characteristics of the distribution of practice (number and length of periods, intervals between periods, degree of learning being considered, etc.); second, the type of material being learned; third, the age of the subjects; fourth criterion or aim of the learning (immediate or delayed recall, speed, accuracy, and amount of recall, improvement, etc.); fifth, the order of repetitions within a practice period (whole vs. part order); sixth, manner of studying; seventh, the stage of learning (whether the distribution is equally effective at the initial and final stages of learning and in the exercise of a well learned habit). Certain other factors or conditions have been indicated but have not received special treatment, such as individual differences, affective value of the learning, motivation, etc. If a thesis is permissible, it is that any statement of the value of distributing or massing learning must be qualified carefully in terms of the conditions listed above in view of

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the instability of the margin of superiority between these two methods of learning.

Since most of the conditions enumerated above enter in some fashion into any investigation of the general problem of the economy of distributing work and rest periods, it has been possible to tabulate most of the investigations in a chart arranged on the bases of these conditions. In this chart the studies, or parts of studies, are listed roughly according to the main variable factor rather than in chronological order. Whenever the significant data permit numerical presentation in a brief compass they are entered under 'Results' in the chart. Certain investigations cannot be forced into the chart at all. They are summarized in the body of the review along with a further consideration of the studies found in the table.

Type of Material to be Learned in Relation to the Characteristics of the Distribution

To avoid unnecessary repetition, the general characteristics of the distribution are treated in connection with the type of material used in the experiment. The type of learning material has been varied for the same distribution in only one experiment, but the comparison of distributions of different extents (distribution will be used as a general term and includes absolute massing) has been the central feature of numerous investigations. The learning schedules differ in the amount of time to be spent or amount and difficulty of the material, the number and length of separate practice periods and whether the periods are decreasing or increasing in length, the length of interval between the periods of practice and whether diminishing or increasing in length, and the period of time over which the learning extends. Investigations intended for school room application usually keep the time interval invariable, and seek to determine the optimal length of period for a 24-hour interval. The interval between periods of learning is of importance in other than its quantitative aspects. The same interval spent in rest, in different types of mental work, physical work, play, etc., may approach the

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Perkins (1914)	4	Adult	Nonsense syllables, 16 repetitions of lists of 7 pairs	1 reading of list 2 readings of list 4 readings of list 8 readings of list	1, 2, 3, and 4 days Same Same Same	Per cent correctly recalled after a two week interval	1 2 3 4 days' interval 1. 79 72 82 68% 2. 43 78 65 45% 4. 25 33 29 41% 8. 9 16 11 17%
Robinson (1920)	12	Adults	12 presentations of 10 3-place numbers	12 presentations 6 presentations	None 1 day	Recall after 5 min., 20 min., and 24 hours, as measured by 1. Total digits 2. Correct digits 3. Time per digit	After 5 min. / After 20 sec. / After 24 hrs. 6–6 12 6–6 12 6–6 12 1. 25.4 24.6 24.8 25.0 21.6 20.3 2. 23.1 20.8 21.3 20.2 15.4 9.6 3. 1.85 2.55 2.15 2.65 2.8 3.4 3–3 6 3–3 6 3–3 6 1. 19.3 20.4 19.1 19.7 14.5 14.0 2. 14.6 16.9 13.8 14.6 8.8 6.8 3. 2.6 2.2 2.2 2.8 3.92 3.95
			6 presentations	6 presentations 3 presentations	None 1 day	Same	
Pieron (1913)	1		Numbers, lists of 20	1 reading of list	30 seconds 2 minutes 5 minutes 10 minutes Varying intervals up to 48 hours	'Perfect mastery'	11 readings required for learning 7.5 readings required for learning 6 readings required for learning 5 readings required for learning . . . about 5 for various intervals 5 between 10 min. and 48 hours

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Lyons (1914)	1	Adult	Nonsense syllables	8, 12, 16, 24, 32, 48, and 72 syllables	1 day	Time in minutes for complete learning	8 12 16 24 32 48 72 104 200 300 .12 1.5 3.6 5 6 14 25 37 93 195
				Same number of syllables learned continuously	None		.25 6 9 16 28 43 138 .. .. ..
Jost (1897)	2		Nonsense syllables, 24 readings	8 readings 6 readings 2 readings	1 day 1 day 1 day	Recall after 24 hours	18 Score for 7 Score for 39 subject 31 subject 53 B. 55 M.
Culler (1912)	5 5 5 5 5 3	22–28 years old	Hampton Court Maze, 12 trials	12 trials 6 trials 4 trials 3 trials 2 trials 1 trial	None 1 day 1 day 1 day 1 day 1 day	1. Av. time—last 3 trials 2. Av. errors—last 3 trials 3. Improvement in time 4. Improvement in errors	Time Errors Time Gain Accuracy Gain 48 3.0 368.7% 147.9% Invalidated through indifference of subject 39 .9 341.0% 218.5% 59 3.2 195.0% 302.0% 61 5.2 253.5% 161.5% 50 4.8 210.0% 147.9%
Pyle (1914)	5	College seniors	Typewriting, 45 hours practice	10 half hour periods	Half hr. between period 1 day every 10th period	1. Per cent superiority of distribution 2. Per cent superiority in accuracy	1 2 3 4 5 6 7 8 9 (1) 23 28 27 39 27 21 23 23 22%
	5			2 half hour periods per day	About 7 and 17 hours alternately	(By successive 5 hours of practice)	(2) 25 12 44 121 161 .. .. .. 223%

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Lashley (1915)	26	14–36 years old	Archery, 360 shots	5 shots 12 shots 20 shots 40 shots 60 shots	1 day 1 day 1 day 1 day 1 day	1. Gain of final 5 shots over initial 5 2. Gain of final 40 shots over initial 40 3. Gain of final 180 shots over initial 180	1 2 3 34.2 19.3 10.9 average inches from 47.6 26.0 11.4 the bull's-eye 19.6 12.3 5.4 26.8 15.5 5.2 29.1 15.7 6.6
Leuba and Hyde (1905)	5 5 4 3	21 years av. age	Writing German script 16 20-minute practices	20 minutes	Twice a day 1 day 2 days 3 days	Av. no. of letters written in 20 minutes after 5, 10, and 16 practices	5 10 15 practices 625 865 1,015 825 1,115 1,540 780 1,175 ** 750 985 **
Starch (1912)	12 14 9 7	College students	Substitution code translation 120 minutes	10 minutes 20 minutes 40 minutes 120 minutes	Twice a day 1 day 2 days None	Average amount done in 5 minutes based on last 15 minutes of practice approximated from graph	262 letters in 5 minutes 250 letters in 5 minutes 195 letters in 5 minutes 130 letters in 5 minutes
Pyle (1913)	6		Substitution 8 hours	30 minutes	1 day 2 days	Each subject compared with a check experiment	Day interval subj. 2 day interval subj. 17.2% loss against 22.6% loss 15.4% loss against 32.3% loss 12.0% gain against 2.1% gain
			Substitution	15 minutes 30 minutes 45 minutes 60 minutes	1 day	†Relative improvement to check experiment (1) Average speed (2) Gain in speed	(1) (2) 22.3% better, 4.9% less than check exp. 36.1% better, 18.1% more than check exp. 25.0% better, 5.4% more than check exp. 14.8% better, 45.5% more than check exp.

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Cummins (1919) (A)	5	Adults	French vocabularies 120 minutes	Equal 20–20–20–20–20–20–20	Equal—about 3 days	Recall after 1 day and after 1 week combined	110 words correct reducing / equal 1.02 111.7 words correct 1.7 favor of reducing schedule 3.56 P.E. of obt. diff.
(B)	42	Normal school students	Addition	Reducing 40–30–20–15–10–5	Increasing—1 to 4 days		
	41		60 minutes	Equal A 10–10–10–10–10–10	2 days	(1) Av. gross gain in no. of correct problems	(1) (2) 9.74 5.00
	36			Equal B 10–5–5–5–5–5–5–5–5–10	1 day	(2) Av. gain in per cent correct of problems attempted (accuracy)	9.36 3.13
	38			Reducing A 10–15–10–7½–5–2½–10	1 day		10.62 5.56
				Reducing B 10–2½–5–7½–10–15–10	1 day		9.92 6.32
(C)	114	7th grade	Geog. 120 min.	15–15–10–10–15–10–10–7½–7½–5–15	Increasing	(1) Av. gross gain in no. of correct problems	(1) (2) 1.12 6.19 Av. of all groups tested: gives a ratio for amount (1) 1.125 with P.E. of obt. av. of 0.033 and for accuracy 2.64 with P.E. of obt. av. of .371
	69	6th grade	Geog. 120 min.	15–15–55–15–15–15–15–55	Equal	(2) Av. gain in accuracy presented by the ratio	1.50 3.28
	328	3–4th grade	Add. 115 min.	Equal in	Equal		1.04 5.46
	127	5th grade	Div. 115 min.	Reducing	Increasing		1.03 1.20
	116	8th grade	Geog. 115 min.	Reducing—15–15–15–12½	Variable, less than 2 days		.93 .88
	111	7th grade	Hist. 115 min.	10–10–7½–7½–5–2½–15		Gain on reduc. schedule	.95 2.60
	141	6th grade	Hist. 115 min.			Gain on equal schedule	1.23 1.35

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Cummins (C) (1919)	122	5th grade	Geog. 115 min.	Equal–10–15–15–15–15–15–10–15	2 days		1.34 2.46
	201	3–4th grade	Add. 115 min.				1.11 1.52
Austin (A) (1913)	1	Adult	Logical	5 readings	None	(1) Recall after 1 day	(1) 66.00% (2) 13.13% (3) 11.49% retained
			5 readings			(2) Recall after 2 weeks	
				1 reading	24 hours	(3) Recall after 1 month	64.40 37.26% 30.59%
(B)	32–53	College students	Logical 3 readings	3 readings	None	Immediate recall	No. 1 No. 2 No. 3 No. 4 group
				1 reading	24 hours		34.0 45.7 49.0 40.2% retained 46.0 45.6 52.4 39.3% retained
Gordon (1925)	101	College students	Logical	6 readings	None	Correct word in correct position (possible 115 points)	Immediate Delayed recall
				3 readings	3 days		80 points 37 points after 1 month
				3 readings	None		76 points 48 points after 1 month
				1 readings	7 days		54 points 26 points after 3 weeks
							45 points 32 points after 3 weeks
Pechstein (1921)		Rats and adult humans	Maze consisting of 4 sections	2 trials per day by sections	1 day	(1) Trials (2) Time in seconds (3) Errors before making 4 out of 5 perfect trials	Rats Humans Trials Time Errors Trials Time Errors
				One section practiced continuously until mastered	1 day		30 1907 199 23 1220 237 10 829 135 10 538 108
				2 trials per day on the whole maze	1 day		27 4174 217 12 641 126
				Learned as a whole on one day	None		Rats incapable of so much continuous work 30 1250 260

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results		
Gopalaswami (1924–25)	10		Mirror star Tracing—	Whole star	3 times per day	No. of trials before completion of 2 circuits of the whole star within 28 strokes and 4 errors of each other	Whole star 747 whole massed	Half star	Quarter star
	10		star divisible into quarters (a–b–c–d)	One quarter of star	12 times per day (3 in coordinating quarters)		735 part massed	375 part massed	222 massed whole
	10			Varied— a–b–ab–c–abc–d–abcd	3 times per day regardless of length of run		694 progressive part distributed 739 two part (massed)	401 part distributed 468 whole massed	296 whole distributed
	10			One half of star	6 runs per day (3 in coordinating halves)				
Carr (1919)	20	College students	Pencil maze	10 trials first day 1 trial a day for 10 days	None for first 10 trials 1 day for 2d 10 trials	1. Errors per 10 trials 2. Time per 10 trials 3–4 perfect trials out of 5	1st 10 trials	2d 10 trials	(3)
			20 trials				1126 (1.43) (massed)	319 (0.98) (distributed)	2 subjects
				1 trial a day for 10 days and 10 trials on the 11th day	1 day for 1st 10 trials None for 2d 10 trials		784 (1.00) (distributed)	327 (1.00) (massed)	6 subjects
							Two methods equal by the time criterion		

Experimenter (Date)	No.	Subjects Age	Learning Activity	Length of the Practice Period	Time Interval between Periods	Criteria of Learning	Results
Mould, Treadwell, & Washburn (1915)	144	College students	4 presentations of a list of 10 nonsense syllables	1 syllable	4 secs. 1 min. (syllable articulated) 4 secs. 1 min. (articulation suppressed) 4 secs. 1 min. ("deb" articulated)	No. of syllables correctly recalled by all subjects after 4 presentations of the list	1,001.0 7.3% advantage for 1 min. rest 1,075.4 847.9 3.7% advantage for 1 min. rest 879.9 902.0 1.9% advantage for 1 min. rest 920.0
McClatchy (1925)	24 39 24 24 39 24 24 15 15 15	College students	Pencil maze Mastery of maze	Continuous 1st trial 3d trial 5th trial 7th trial 9th trial 11th trial 3d trial 5th trial 9th trial	48 hour rest interval occurring after trial indicated in previous column 24 hour interval inserted	(1) Av. no. of trials (2) Av. amount of time (3) Av. no. of errors before mastering the maze	Trials Time Errors 20 751 96.4 27 1,136 103.7 26 1,133 108.4 24 1,111 207.0 17 721 82.0 20 970 148.0 22 1,029 90.0 17 632 96.4 19 685 105.8 24 879 113.0

optimal interval for a period of a given length to entirely different extents. The same amount of work, the same number of periods, and the same number of days included within a distribution leaves the lengths of the periods and the lengths of the intervals undefined, since they may be either equal, increasing, or decreasing in length throughout the course of the practice.

Nonsense Materials.—Lyons (**20**) has made the only study of widely varying materials with the same distribution. He conducted an extensive and heroic series of experiments on the relation of the length of study periods to the advantage of distribution, in which he memorized varying amounts of nonsense syllables, digits, prose, and poetry at one sitting and at the rate of one repetition per day. There was only one subject and one determination for each length of period, but when several successive periods are considered together enough learning is involved to be significant. The data for the nonsense syllables are presented in the chart. For both nonsense syllables and digits, the shorter lists show a consistent saving in time by distributing the memorizing, and for the larger units of work spacing practice is overwhelmingly economical. There appears to be no consistent saving in time by distributing the memorizing of the logical materials, prose and poetry. Within the limits explored by Lyons, the factor of the amount of material influences the economy of distribution when the learning is rote, but not when it is logical.

Perkins (**28**) presents data which permit a comparison to be made between the relative influence of the length of the study period and the interval elapsing between study periods on the economy of distributing effort. Sixteen repetitions of lists of nonsense syllables are divided into periods consisting of 1, 2, 4, and 8 readings of the lists with 1, 2, 3, and 4 day intervals for each period length, making 16 different distributions of the learning involved. In general, less was recalled after a lapse of two weeks when the readings occurred every day than when separated by an interval of 2 to 4 days for all lengths of periods concerned except the period consisting of

one repetition per day, which shows the least recall for study every other day.

The percentage of syllables correctly recalled was inversely proportional to the length of the study period regardless of the time interval between periods. Within the limits of the experiment, varying the length of the practice period has more influence on the amount recalled than does varying the interval between practices. Averaging the per cent of syllables correctly recalled for each period length regardless of interval, gives for periods of 1, 2, 4, and 8 repetitions 75, 58, 32, and 13 per cent correctly recalled respectively. The average of all lengths of periods for intervals of 1 to 4 days gives respectively 39, 45, 47, and 45 per cent correctly recalled. The length of period seems to be the more significant factor for the learning of nonsense materials for recall after two weeks. Piéron (**29**) finds an approximately equal superiority for different time intervals between practices varying between 10 minutes and 48 hours in length over a 30 second or a 5 minute interval. Here, again, beyond a certain point the interval between practices of equal length fails to be of importance.

The economy of two simple distributions (continuous work and equal division of the work between two successive days) has been investigated by Robinson (**33**) for small amounts of learning (12 and 6 presentations of lists of numbers) as judged by recall after 5 minutes, 20 minutes, and 24 hours, according to three criteria, total number of digits recalled irrespective of correctness, number correct as to content and position, and rapidity of recall. Nine criteria are afforded for the comparison of the two types of distribution. When 12 presentations constitute the degree of learning, 8 out of the 9 criteria show an advantage for the distributed study, but when 6 presentations or one half the amount of study is concerned the concentrated study is favored by 5 out of the 9 criteria. The advantage of this particular distribution of rote learning appears to vary with the total amount or degree of learning involved as judged by amount, accuracy, and rapidity of recall after varying periods of time.

Motor Habits.—Twelve trials on a Hampton Court Pencil

Maze are distributed by Culler (7) into different lengths of periods with a constant interval between practices, except, of course, for absolute massing. The lengths of period are 12 trials with no interval between and 6, 4, 3, 2 and 1 trials per day with one day elapsing between periods, necessarily making the time spanned by the learning unequal and the time length of periods variable for the different stages of the learning. Since there is a gradual decrease in the time required for one trial as the learning progresses, making a trial the unit of work amounts to using a decreasing length of period. Outside of the data for the 6 trials per day period (which were invalidated by the indifference of one subject) the one and two repetitions per day groups showed a lower absolute attainment and less improvement than did the 12 and 4 trials per day groups. For the maze used it seems that prolonged and persistent practice repeated for a few days is preferable to shorter practice periods distributed over a longer span of time.

The study of Pyle (32) on the relative economy of two distributions for learning to typewrite is adequately summarized in the chart. The group for which the practice was distributed maintained about a 25 per cent superiority in speed to the massed practice group throughout the whole course of the practice and was from 12 per cent up to 223 per cent more accurate. The group which worked under distributed conditions was, perhaps, a little higher in initial motor capacity. He does not mention whether the learning proceeded by the whole or part method but presumably the part method of some sort was used in view of the nature of the habit involved. Pechstein (25) found that rats were incapable of sufficient continuous work to learn the maze in question as a whole in one day, but learned it in 27 trials at the rate of two trials per day, also by the whole method. Thirty trials were required by humans to learn the maze as a whole at one continuous sitting, while it was learned in twelve trials when the trials were separated by one day intervals. He finds a different result, however, when the maze is attacked by the part method, in which case the distributed practice is found to be less economical. Leuba and Hyde (19) in an early study in

which the subjects practiced the hand movements requisite to writing German script, found that 20 minutes of practice every day or every other day yields a better gain than either more highly concentrated or more highly distributed practice, *i.e.*, two 20-minute periods a day or 20 minutes of practice every third day. Lashley (18) practiced subjects on archery at the rate of 5, 12, 20, 40, and 60 shots per day for 360 shots at the target. During the first 180 shots there was little or no relation between the extent of distribution and improvement in accuracy of shooting, but during the last half of the practice the extent of distribution is associated with greater proficiency. Murphy (24) found that practice at throwing a javelin at a target is best distributed to the extent of one or three practices a week rather than practice 5 times a week.

Code Translation.—The much quoted experiment of Starch (35) involving the distribution of 120 minutes of practice time into periods of 10, 20, 40, and 120 minutes with different time intervals for each length of period, favors the shorter periods. Graphical presentation alone is supplied so that the data used in the chart are approximations from a graph. Both the length of period and the time interval between are varied, and there is no mention of equating the groups, which varied from 7 to 14 subjects. The group working in 10-minute periods translated an average of 10 letters per 5 minute period more than did the group working 20 minute periods, which group in turn translated an average of 21 letters more per each 5 minute period than did the 40 minute group. Similar code substitution material was used by Pyle (31) to determine whether 30 minutes of practice every day is more efficient per unit of time than the same amount of practice on alternate days. The two methods of distribution are used by a pair of subjects and their results are compared with a check experiment which is the same for each pair of subjects. The three comparisons thus afforded all favor daily practice. Another such determination favors daily practice for a 20 minute period, and another indicates that practice daily and on alternate days are equally efficient when the length of the period is 11 minutes. Dearborn (9) in a brief experiment

reports that 10 minutes of working on substitution once a day yields better results per unit of time than 10 minutes of practice twice a day, and Munn (**23**) in an experiment on the same learning activity favors distribution of effort into very short periods of practice, less than one minute. The studies indicate that for code translation distributed practice favors learning more than massing does.

Logical Materials and School Subjects.—Under this heading will be summarized investigations in which the material is logical with recall tested for a verbatim reproduction, in which the material is logical and the recall tested for ideas contained, and in which the learning consists in drill on some arithmetic function. Lyons (**20**) parallelled his experiments with digits and nonsense syllables by using prose and poetry as learning materials, and found that for various lengths of material there was no consistent advantage or disadvantage in studying at the rate of one repetition per day instead of entirely learning the passage in one sitting. One determination was made for 20 passages of poetry in graded lengths between 25 and 2,500 word passages, and for 20 passages of prose varying in length between 15 and 1,200 words. Gordon (**13**) recently has carried on some class experiments in which the learning of a meaningful passage was tested by the ability of the subjects to recall it word for word. Six repetitions of the passage on one day proved about equal to two periods of 3 presentations separated by a 3 day interval for immediate recall, but was considerably inferior when the criterion was recall after the lapse of one month. Three presentations with a week interval between each one was less economical for immediate recall than the same amount of time spent continuously, but was more economical for delayed recall (3 weeks). Austin (**1**) finds that with logical materials, repetitions lose in effectiveness if too close together or too widely separated. (See below under Criterion of Learning.)

Kirby (**17**) attacks the problem of the most favorable length of period when there is an interval of one day between periods of addition and division. 2 minutes per day is superior to longer periods. The results have been criticized by

the investigator, inasmuch as the students studying 2 minutes per day would be studying over a greater period of time, giving more opportunity for outside practice and learning through the regular school work. Hahn and Thorndike (14) have investigated the identical problem, using addition as the learning material. A 22½ minute period is superior to a 11¼ minute period for 7th grade students and a 10 minute period is about equally superior to a 5 minute period for the 4th grade, while for the 6th grade 20 minutes is just slightly superior to 10 minutes and 7½ and 15 minutes are equally efficient for children in the fifth grade.

Thorndike (39), using multiplication drill, finds a case in which a given amount of work per day is done more advantageously at one continuous sitting than if distributed into 4 sittings on the same day. Also he finds that whether practice is spread over 24 days or is consolidated into 6 makes little difference, provided the long day's work is made at one sitting. With 2 units of work per sitting and 4 sittings per day, and with 8 units to a sitting and one sitting a day, there is little difference between practice daily or on alternate days. It proved more advantageous to introduce a rest period of 10 minutes between two series of five mental multiplication problems than a 20 minute interval or no interval at all. Edwards (11) compared groups who studied various school materials, geography, history, and others, 6½ minutes continuously or with the same amount of time divided into a study period of 4 minutes followed some days later by a review of 2½ minutes. Time intervals, difficulty of the subject matter, age of the subjects were so variable that it is difficult to interpret the results, which tended to favor the spaced study.

An extensive series of experiments in which an elaborate distribution schedule is compared with a schedule of periods of equal length, has been carried on by Cummins (8). A total of 120 minutes of study was distributed into periods of 40, 30, 20, 15, 10, and 5 minutes, with an increasing interval of time between, and into six 20-minute periods with equal intervals (3–4 days) between. These two distributions are comparable

in that they both involve the same amount of time and the same number of periods, and substantially span the same number of days, and differ only in the length of the study periods and the intervals between periods. The ratio representing the superiority of the groups on the schedule of periods diminishing in length over the groups on the equal schedule, in terms of the average gross gain, gives for all ages and materials an average ratio of 1.125 with a P. E. of 0.033. The accuracy criterion gives the diminishing schedule a greater, statistically significant superiority, the diminishing of equal ratio being 2.64 with a P. E. of .371. These results will be considered in more detail under another heading. Cummins (B on the chart) gives data for comparing two 'equal' distributions of varying degree of massing. Sixty minutes of study divided into six 10-minute periods is just as economical as the same amount of time divided into an initial and final test period of 10 minutes separated by eight 5-minute periods when the criterion is average gross gain, and distribution of effort into smaller periods is slightly unfavorable if the gain in per cent of problems correct is the criterion. The subjects were about 20 years of age and were practicing addition, which must be taken into account in interpreting results.

Age of the Subjects

There is no direct attempt to ascertain the influence of the age of the subjects on the relative economy of distributions varying both in type and extent. Nevertheless age is in all probability a factor. The majority of the findings have been based on experiments in which the subjects have been at least of college age, with the exception of those in which school materials have been used, in which case the subjects are appropriately of school age. Edwards (11) used grade and normal school subjects, and suggests that distribution may be more important for children than adults. The results are not of much assistance because of the failure to control conditions carefully. Kirby's investigation (17) favors very short periods of work for young children, but the advantage is probably exaggerated by the operation of other causes. The

experiments of Cummins (8) offer the only example of the same distribution being tested out with subjects of different age, but his results do not furnish the basis for adequate comparisons, since the lower grades worked largely at drill in arithmetic functions and the upper grades dealt with more logical material. Drawing an average from the third, fourth, and fifth grade ratios as shown in the chart and comparing them with the average of the three higher grades, reveals no very significant differentiating advantage in distributing practice for either age group. This holds for both criteria. Adults learned French vocabularies equally well under the two schedules described above. Further, for normal school subjects working on addition there was no great difference between two distributions of the same type but of different extents, and it seemed to make little difference whether the lengths of periods increased or decreased throughout the course of the practice. The greater fatigability of children, their lesser perseverence, etc., indicate that age is possibly an important factor here and that optimal conditions for studying various sorts of materials should be worked out for different ages.

Criterion of Learning

The object, aim, goal, or criterion of learning is a factor to be considered in deciding the superiority of massed and distributed practice. The various criteria which appear in the investigations are free and assisted recall, after periods of time varying from immediate recall to recall after a month interval, as measured by amount, accuracy, and rapidity. Where drill in arithmetic or any skill in which the subjects have previous training and varying abilities is concerned, learning is measured by a comparison of initial and final attainment. The formation of a muscular habit can be measured by final attainment or by gain over initial performance. On the whole the effects on retention of distributing practice in the latter learning activities have been neglected. However the recall is measured, the extent to which it is delayed is perhaps the most important factor in differentiating spaced and unspaced work in regard to economy.

A striking demonstration of the importance of the period separating the cessation of practice and the testing of the learning is found in the study of Austin (1). In terms of the percentage correctly recalled, one reading of the material per day for 5 successive days was 1.6 per cent inferior to 5 continuous readings for immediate recall, 24.13 per cent superior for recall after two weeks, and 19.10 per cent superior for recall delayed one month. This is the margin of superiority and not the difference in absolute per cent correctly recalled. The two distributions are about equal for immediate recall, but the distributed work yields results for recall after two weeks or a month about three times as great as the continuous study. The same scheme of distribution for 3 readings of a textbook assignment showed no significant advantage in distributing effort when immediate recall is the goal of learning. A recent study by Gordon (13) substantiates the findings of Austin. Six readings of a passage on one day was slightly superior for immediate recall to 3 readings a day separated by a three day interval, but the spaced readings were pronouncedly more advantageous for recall delayed one month. Likewise, three continuous readings were decidedly more profitable for immediate recall than three readings separated by a week interval, but the advantage was reversed when the recall was measured after the lapse of three weeks.

	Delay of Recall			
	5 min.	20 min.	24 hours	Total
No. of times 6 readings is superior	37	31	46	114
" " " 12 " " " "	21	27	15	63
" " " neither is superior	14	14	11	39
" " " 3–3 readings is superior	21	33	36	90
" " " 6 " " " "	45	29	26	100
" " " neither is superior	6	10	7	23

The most elaborate scheme of criteria is used by Robinson (33) in that he tested recall after 5 minutes, 20 minutes, and 24 hours by amount, accuracy, and speed of recall. Robinson

has analyzed his data in order to bring to light the factor of delayed recall, by counting the number of times (by individuals) each distribution is superior on the basis of a composite of the three criteria. A division of 12 presentations into two equal periods 24 hours apart is advantageous for recall delayed 24 hours, but not to any great extent for recall as immediate as 5 or 20 minutes. When 6 presentations are used the continuous work is the more advantageous for immediate recall, but not so for delayed recall. The three studies on this phase of the question are univocal.

In Robinson's study, distribution of learning appears to a better relative advantage when learning is measured by correct digits and speed of recall than when measured by amount of recall. The possibility that distribution makes a greater difference in accuracy than in amount, is indicated in several investigations. Hahn and Thorndike (**14**) weighted a wrong answer in two different ways and thereby altered the superiority of certain lengths of periods over others. Cummins' (**8**) accuracy criterion reveals a much greater advantage for the 'reducing' distribution (*i.e.* the more highly distributed practice) than does the amount criterion. In learning to typewrite, Pyle (**32**) found a considerable superiority in speed for the more highly distributed schedule (between 20 and 30 per cent for successive stages), but a considerably greater superiority in accuracy in the initial stages of learning and an overwhelming superiority in the final stages. Also, the error criterion and the meeting of 4 errorless trials out of five differentiate two sorts of distribution which are equally effective as judged by the time criterion in Carr's study (**6**). Two studies using logical learning, one using rote, and two involving motor learning, indicate that distribution is most economical in avoiding errors rather than in making for rapid learning. This fact offers a clue to the explanation of why distribution is or is not favorable to learning.

Order of Repetitions within the Periods

The intimate relation of the problem of the whole vs. part method of learning a motor act to the problem of the economy

of interspersing work and rest periods has been demonstrated by Pechstein (25, 26, 27). Previous researches all concerned learning in which the unit was repeated as a whole. Pechstein found that when the maze was attacked as a whole it could be learned in fewer trials, less time, and with fewer errors by humans when the practice was distributed at the rate of two trials per day than when the practice was massed into one day. In the face of such a complex situation rats were incapable of sufficient continuous effort to learn the maze at all under massed conditions, but were able to do so when the learning was distributed at the rate of two trials per day. Massing effort seems unfavorable to the whole method of learning. The maze which he used for both rats and humans was divisible into four smaller and unitary sections to permit part learning. Each fourth of the whole maze was learned separately, and finally the parts were run serially until the subject was able to thread the maze as a whole. A combination of part method and massing effort was obtained when the four sections were learned separately on four successive days and were integrated on the fifth, and a combination of part and distributed practice was obtained when the parts were learned and joined at the rate of two trials per day. Under part conditions the massed practice proved the more efficient for both rats and humans. Of the four different ways in which the maze was learned, the union of massed practice with the part method proved the most efficient. When the units had been learned at the rate of 2 trials per day, the act of connecting them together proved difficult, but when the maze had been learned at the rate of one section per day the complex act of connecting them was comparatively simple. The rats were capable of exerting a surprising amount of continuous effort on the simple unit, making a high degree of massing possible. Pechstein interprets the superiority of massed effort combined with the part method in hard problems to lie in preventing confusion, hesitation, emotional conditions, and in being more suited to the organism's powers.

Gopalaswami (12) has investigated the same problem with mirror tracing, using a star which could be divided into

smaller comparable units. For the whole method massed, 3 runs were made per day on the star as a whole; for the pure part method massed the star was divided into 4 similar and equally different parts which were learned successively at the rate of 12 trials per day and finally the whole star was practiced at the rate of 3 runs per day; the progressive part method with distributed practicing, in which the parts are learned separately but are immediately joined to those learned previously, involved 3 runs per day irrespective of the length of time required for each run. The two-part method divided the star into halves which were first learned separately at the rate of 6 trials a day and then joined. There is a considerable superiority for the progressive part method with distributed practice. The pure part method with massed practice is somewhat superior to the whole method with massed practice, which is in agreement with Pechstein's findings. By considering only the first half of the star, the comparisons afforded show that distributing effort is somewhat unfavorable to part learning, and that the part method, regardless of how the practice is distributed, is considerably superior to the whole method with massed practice. For just one quarter of the star continuous work is more advantageous than distributed work.

Manner of Studying

That the manner of studying or presenting memory material may affect the relative economy of continuous and spaced learning is suggested by one study, that of Mould, Treadwell, and Washburn (22). The advantage of allowing 4 seconds interval to elapse between presentations of nonsense syllables as compared with a one-minute interval was ascertained for three different methods of studying the list, one in which the syllable is articulated, another in which the articulation is suppressed, and a third in which 'deb' is pronounced instead of the syllable which is being presented. The longer interval between syllables was 7.3 per cent superior when articulation was permitted, 3.7 per cent superior when suppressed, and 1.9 per cent advantagous when 'deb' was pronounced. The economy of distribution to the extent that

a single movement or a single presentation of a syllable or word is followed by a brief pause has been investigated. Bergstrom (4) in presenting lists of nonsense syllables found that employing longer intervals between presentations of single syllables makes for more efficient learning than where no interval or a very short one is allowed. The same holds for intervals between series that make up a study period. The opposite result is reported by Bean (3) who studied the advantage of different intervals between single throws at a target. He found an inverse proportion between the interval separating throws and improvement. Browning, Brown, and Washburn (5) compared motor learning in which the requisite movements were made continuously and in which they were separated by a pause of one minute. The one-minute period of inactivity was more favorable to learning than no interval, and was particularly so on the more difficult series of movements.

Period in the Learning Curve

That the various criteria of learning for a given material, type of distribution, etc. show a superiority for either distribution or massing effort in learning some material or habit completely, does not permit the conclusion that one or the other type of distribution represents the optimal efficiency. The possibility exists that distribution may be more effective during the initial stages of habit formation or learning of any sort and massing more efficient in the final stages, or vice versa, in which case a combination of a greater degree of distribution at one period and a lesser degree at others would produce the optimal arrangement of work and rest periods. Carr (6) has studied this problem specifically by using two distributions, one in which the first ten trials are continuous and the second ten trials separated by day intervals, and one in which the arrangement is reversed. When the first ten trials of one schedule (massed) is compared with the first ten trials of the second schedule (distributed) the errors of the two series of ten trials are to each other as 1.43 is to 1; the errors of the last ten trials of the first schedule (distributed)

are to the last ten trials of the second schedule (massed) as 0.98 is to 1. In regard to errors (which are probably most affected by distribution), the initial practices should be spread over a long period of time, and it seems to make little difference whether the practice is spaced or not in the final stages of the learning. The time criterion shows equality for the two schedules, but a criterion of 4 perfect trials out of 5 was met three times as often by the subjects who practiced under distributed conditions in the initial stages. Lashley (**18**) in his investigation of the relation between the degree of distribution and improvement in archery, finds about the same improvement regardless of the degree of distribution in the first half of the total practice, but in the last half greater distribution means greater improvement. During the first periods in acquiring a new motor skill without instruction, there are many sudden improvements through 'insight' which are unrelated to distribution, which accounts possibly for the failure of the relation between the spacing of practice and improvement to appear. Pechstein (**25**) believes with Carr that distributing effort is of value for the exploratory and eliminative stages, and massing advantageous for the mechanizing stage. Pyle (**32**) analyzes his data on typewriting to bring out any relation between the period of the learning curve and the superiority of distribution, by making his comparisons on the basis of successive 5 hours of practice. The spaced learning maintains a fairly consistent margin of superiority over the unspaced in speed, but for accuracy the spaced practice is increasingly superior period by period. The accuracy criterion favors distribution in the later stages of habit formation more than in the initial stages. Pyle (**30**), working with a substitution test, reports some evidence that in the early stages of habituation the second practice on the same day gives good returns, while in later stages alternate days may be the best distribution.

Recently McClatchy (**21**) has explored the stages of learning a pencil maze for the optimal position for introducing a rest period. She interpolated a 48-hour rest period after the first, third, fifth, seventh, ninth, and eleventh trials for

corresponding different groups, and compared them on the basis of trials, time, and errors for learning. The criterion indicates that the optimal locus for a 48-hour period of rest is after the seventh trial, and that the insertion of the rest at any other point is detrimental. Inserting a 24-hour rest is beneficial if it is located between the first and fourth trials and detrimental only after the ninth trial. The proper position of the rest period depends on its length, that is, no general statement as to the benefits of massing at any point in the learning curve can be made.

Jost (16) found that the optimal distribution is obtained by massing the early learning, which conflicts with the view of Steffens (36) who found that learning is facilitated by distribution in the first stages. Cummins (8) gets at the same problem by comparing a distribution consisting of periods and intervals of equal length with one in which the periods decrease in length and the intervals increase. The practice is more highly massed at first in regard to both the length of periods and the frequency with which they occur. A large number of determinations show massing in the initial stages to be somewhat advantageous if the criterion is amount, and much more so if the criterion is accuracy. In another phase of the experiment, Cummins compared 60 minutes of time distributed so that the periods diminished in length with a schedule in which the periods increased in length, all other conditions being equal. The subjects were adults and practiced addition. It seemed to make little difference here if the concentrated practice came first or last. The inconsistency of this phase of the experiment with results obtained by comparing the equal and diminishing schedule may possibly be due to the age of the subjects and to the material used, which was an arithmetical function previously practiced considerably by them, so that the complete learning process was not represented. It is difficult to see why the diminishing schedule should be superior to an 'equal' schedule but not correspondingly superior to an 'increasing' schedule.

The investigators who present data on this point are far from being agreed. Robinson (33) offers data obtained by

comparing the relative advantage of massing and distributing effort when the material is to be learned with different degrees of thoroughness. When a list of numbers to be memorized was presented 12 times, 8 out of the 9 criteria indicated that the best results were obtainable by having 6 presentations on two successive days rather than all 12 at the same sitting. Only 4 out of the 9 criteria favor a distribution of 6 presentations in the same fashion. The difference may be due to the difference in work-period lengths necessarily involved, or to the different degree of learning involved, in which case distribution is favored in the final periods.

Robinson (**34**) raises the question as to the advantage of spaced exercise of a well learned habit. He refers to an example of an industrial situation reported by Jones (**15**) in which the normal rate of a group of workmen, the production of 16 pieces per hour by continuous work, was increased by interspersing rest periods as follows:

Work Period	Rest Period	No. of Pieces per Hr.
All day	None	16
25 min.	5 min.	18
17 min.	3 min.	22
10 min.	2 min.	25

In a similar case, previously continuous work at driving rivets was divided into one-and-three-quarter minute periods separated by 2 minutes of rest, with the result that the total amount accomplished was raised from 600 to 1600 rivets per day. That resting 5 hours and 20 minutes out of a 10 hour work day increased the output 266 per cent illustrates the importance of this whole question in industrial situations.

BIBLIOGRAPHY[1]

1. Austin, S. D. M., 'A Study in Logical Memory,' *Amer. J. Psychol.*, 1921, 32, 370–403.
2. Awaji, E., 'The Effect of Length and Distribution of Rest Periods on the Efficiency of Telegraphic Operations,' *Jap. J. Psychol.*, 1924, 2, 1–45.
3. Bean, C. H., 'The Curve of Forgetting,' *Arch. Psychol.*, 1912, No. 21.

[1] A few references are included here which have not been mentioned in the review.

4. Bergstrom, J. A., 'Effects of Changes in the Time Variables in Memorizing, together with Some Discussion of the Technique of Memory Experimentation,' *Amer. J. Psychol.*, 1907, **18**, 206–238.
5. Browning, M., Brown, D. E., and Washburn, M. F., 'The Effect of the Interval between Repetitions on the Speed of Learning a Series of Movements,' *Amer. J. Psychol.*, 1913, **24**, 580–583.
6. Carr, H., 'Distribution of Effort,' Psychol. Bull., 1919, **16**, 26–28.
7. Culler, E. A., 'The Effect of Distribution of Practice upon Learning,' *J. Phil., Psychol., etc.*, 1912, **9**, 381–383.
8. Cummins, R. A., 'Improvement and the Distribution of Practice,' *Teachers' College, Columbia Univ. Contrib. to Educ.*, 1919, No. 97.
9. Dearborn, W. F., 'Experiments in Learning,' *J. Educ. Psychol.*, 1910, **1**, 373–388.
10. Ebbinghaus, H., 'Ueber das Gedachtniss,' Leipzig, 1885.
11. Edwards, A. S., 'The Distribution of Time in Learning Small Amounts of Material,' 'Studies in Psychology: Titchener Commemorative Volume,' Worcester; Wilson, 1917, 209–213.
12. Gopolaswomi, M., 'Economy in Learning,' *Brit. J. Psychol.*, 1924–25, **15**, 226–236.
13. Gordon, K., 'Class Results with Spaced and Unspaced Memorizing,' J. Exper. Psychol., 1925, **8**, 337–343.
14. Hahn, H. H., and Thorndike, E. L., 'Some Results of Practice in Addition under School Conditions,' *J. Educ. Psychol.*, 1914, **5**, 65–84.
15. Jones, E. D., 'Administration of Industrial Enterprises,' New York, Longmans, Green, 1919.
16. Jost, A., 'Die Assoziationsfestigkeit in ihrer Abhangigkeit von der Verteilung der Wiederholungen,' *Zsch. f. Psychol.*, 1897, **14**, 436–472.
17. Kirby, T. J., 'The Results of Practice under School Conditions,' *Teachers' College, Columbia Contrib. to Educ.*, 1913, No. 58.
18. Lashley, K. S., 'The Acquisition of Skill in Archery,' *Papers from the Department of Marine Biology of the Carnegie Institution of Washington*, 1915, **7**, 105–128.
19. Leuba, J. H., and Hyde, W., 'An Experiment in Learning to Make Hand Movements,' Psychol. Rev., 1905, **12**, 351–369.
20. Lyon, D. O., 'The Relation of Length of Material to Time Taken for Learning and the Optimum Distribution of Time,' *J. Educ. Psychol.*, 1914, **5**, 1–9, 85–91, 155–163.
21. McClatchy, V. R., 'The Optimal Position of a Rest Period in Learning,' J. Exper. Psychol., 1924, **8**, 251–277.
22. Mould, M., Treadwell, L., and Washburn, M. F., 'The Influence of Suppressing Articulation on the Favorable Effect of Distributing Repetitions,' *Amer. J. Psychol.*, 1915, **26**, 286–288.
23. Munn, A. F., 'The Curve of Learning in Studies in Development and Learning,' *Arch. Psychol.*, 1909, No. 12.
24. Murphy, H. H., 'Distribution of Practice Periods in Learning,' *J. Educ. Psychol.*, 1916, **7**, 150–162.
25. Pechstein, L. A., 'Whole versus Part Methods in Motor Learning: A Comparative Study,' Psychol. Monog., 1917, **23**, No. 99.
26. Pechstein, L. A., 'Alleged Elements of Waste in Learning a Motor Problem by the Part Method,' *J. Educ. Psychol.*, 1917, **8**, 303–310.
27. Pechstein, L. A., 'Massed versus Distributed Effort in Learning,' *J. Educ. Psychol.*, 1921, **12**, 92–97.

28. PERKINS, N. L., 'The Value of Distributed Repetitions in Rote Learning,' *Brit. J. Psychol.*, 1914, 7, 253–261.
29. PIÉRON, H., 'Recherches experimentals sur les phenomenes de memoire,' *L'année psychol.*, 1923, 19, 91–193.
30. PYLE, W. H., 'Economical Learning,' PSYCHOL. BULL., 1913, 10, 73.
31. PYLE, W. H., 'Economical Learning,' *J. Educ. Psychol.*, 1913, 4, 148–158.
32. PYLE, W. H., 'Concentrated versus Distributed Practice,' *J. Educ. Psychol.*, 1915, 5, 247–258.
33. ROBINSON, E. S., 'The Relative Efficiencies of Distributed and Concentrated Study in Memorizing,' J. EXPER. PSYCHOL., 1921, 4, 327–243.
34. ROBINSON, E. S., 'Mental Work,' PSYCHOL. BULL., 1921, 18, 456–482.
35. STARCH, D., 'Periods of Work in Learning,' *J. Educ. Psychol.*, 1912, 3, 209–213.
36. STEFFENS, L., 'Experimentell Beitrage zur Lehre vom okonomischen Lernen,' *Zsch. f. Psychol.*, 1900, 22, 321–283.
37. STRONG, E. K., 'Two factors which Influence Economical Learning,' *J. Phil. Psychol., etc.*, 1914, 11, 124–131.
38. STRONG, E. K., 'The Factors Affecting a Permanent Impression Developed through Repetition,' J. EXPER. PSYCHOL., 1916, 1, 319–338.
39. THORNDIKE, E. L., 'Notes on Practice, Improvability and the Curve of Work,' *Amer. J. Psychol.*, 1916, 27, 550–565.

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AN INTERPOLATION PROCEDURE FOR CALCULATING THRESHOLDS

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The technical possibilities of psychophysical limens as experimental indicators of related phenomena serve to give point to investigations aimed at simplifying the procedure for obtaining them. Any considerable interest or belief in an external stimulus threshold of constant value for a particular modality has long since been replaced by the view that the observed sensory threshold is a variable depending for its value upon an unknown number of external and internal factors. Studies of the variations of the threshold when different conditions are experimentally varied should shed some light on the complex of factors determining it and thereby increase its indicative value. If under experimental control the value of a limen is found to be some function of another factor, clearly it can serve as an indicator of the same. And if the indicated factor is relatively difficult to observe directly an easy threshold determination would emphasize the practical utility of the indicator and facilitate research.

The Problem

A previous study tends to show that the constant process of computing thresholds is unnecessarily elaborate.[1] A simpler procedure consisting merely in the interpolation of the limen on the assumption of normally distributed data was found usually to give a value approximating quite closely the constant process value.[2] This interpolation, furthermore, could be performed without special training and required but

[1] *Amer. J. Psychol.*, 1927, 38, 390–402.

[2] In this paper the term 'constant *process*' stands for the usual elaborate procedure of calculating the limen from data secured by experimentation with the related '*method* of constant stimuli.' *Cf.* Thomson, G. H., *Brit. J. Psychol.*, 1912–13, 5, 203 *ff.*

two or three minutes of time. The purpose of the present study is to test the feasibility of that procedure by: (1) calculating limens from new data and again applying the constant process as standard of comparison, in order to secure a relatively direct check on the earlier investigation, (2) comparing various interpolated limens with each other to obtain empirical evidence of their reliability and to gain suggestions concerning a suitable measure of their variability, and (3) comparing interpolated limens based on experimentation with different numbers of comparison stimuli to estimate the comparative applicability of the procedure to data based on as few as two or three stimuli.

The Data

An experiment in lifted weights was performed to meet the requirements of the problem. The weight-lifting procedure, as the considerable literature shows, has become rather well standardized. This fact is of consequence in that it tends to make the present results more strictly representative. Also, it makes a detailed statement of the lifting procedure seem unnecessary, though it may be desirable for comparison.[2] In

[2] The subject sat in a comfortable chair with a padded arm-rest beyond which his right hand extended. The right arm passed without contact through a hole in a screen which excluded from view weights, turntable, metronome and experimenter. The surface of the turntable was sufficiently under the height of the arm-rest to allow the subject's hand to hang down easily in grasping the weights. The weights were grasped with the thumb and fingers and raised about 3 cm. by a wrist motion timed with the metronome. The latter was set at 82 beats with the bell on every fourth. The subject grasped the standard weight on the bell signal, raised it on the next beat, lowered it on the third, and raised his empty hand on the fourth. Between the fourth and the next bell the experimenter turned the comparison stimulus into position; and it was then lifted on the bell in the same manner as the standard. There followed a four beat interval in which the subject reported and the experimenter recorded the judgment. Subsequent pairs of stimuli were lifted in accordance with the same procedure. After every 50 or 60 judgments the subject rested for several minutes.

The turntable made no audible sound and its surface was adequately padded to preclude auditory cues from the noise of the return of the lifted weights. The weights themselves were hard rubber cylinders loaded with lead and solder, and approximately 33 by 33 mm. in size. They were anhygroscopic and did not vary markedly in temperature nor essentially in mass. The standard was always 100 grm. and two sets of comparison stimuli were used (1) 88, 92, 96, 100, 104 and (2) 92, 96, 100 grm. The standard stimulus was always presented first; therefore the time error may be regarded

general the subjects of the experiment were students of psychology—one undergraduate and five graduates—and each served in seven sessions occurring on different days. The subjects were moderately remunerated and appeared to comply with the instructions to the best of their abilities. Prior to this experiment, two of them had had a limited amount of practice in the lifting procedure in other laboratories while the other four had had none. In the present study all subjects had a preliminary practice session followed by brief warming up periods at the beginning of each experimental session. Following the practice session each subject passed 300 experimental judgments per day or 1,800 in all. Four of the subjects lifted 5 comparison stimuli in their odd-numbered sessions and 3 in their even; while the other two subjects lifted 3 comparison stimuli throughout. The series for each subject were so recorded as to give two temporally mixed and therefore comparable groups of judgments; and the series in both of these groups were fractionated. These arrangements made possible the comparisons outlined in the preceding paragraph. The summary of the relative frequencies of subject number 1, given in Table I., illustrates the form and nature of the basic data.

The precision of these data, as indicated by the values of h, on the whole runs slightly lower than that of several extended weight-lifting studies by other experimenters. There are three probable reasons for this: (1) the comparative inexperience of our subjects,[4] (2) the smaller numbers of series in the present study,[5] and (3) our slower rate of lifting.[6] Inexperi-

as constant. The weights to be lifted were always brought to approximately the same position below the subject's hand; hence the space error may be regarded as eliminated. The order of presentation of the stimulus pairs was arranged to compensate the contrast effect discovered by Fernberger. The categories of judgment were 'heavier,' 'equal' and 'lighter,' and the subject was asked to include in the 'equal' category judgments too doubtful to be included in the others.

[4] *Amer. J. Psychol.*, 1927, 38, 390, footnote 6.

[5] Culler points out that precision, h, cannot be high unless degree of normality, P, is. *Cf.* J. Exper. Psychol., 1926, 9, 175. The greater the number of series, n, other things equal, the higher we should expect P to be. Therefore, according as n, and hence P, is relatively low, h is more likely to be so.

[6] This third reason also requires a word of explanation. The metronomes of

ence may be the most potent factor, since the precision of our two practiced subjects was approximately the same as that in the other investigations while the precision of the others ran lower. The fact of somewhat lower precision is mentioned because the difference between simultaneously secured constant process limens, presumably some inverse function of precision, is used as a standard of comparison in the present study. When this difference is relatively large, comparison values will appear relatively small.

TABLE I

SUMMARY OF RELATIVE FREQUENCIES OF SUBJECT I

Category of Judgment			Lighter					Heavier				
Order of Sessions	Number of Series	Number of Judgments	Stimuli *					Stimuli *				
			88	92	96	100	104	88	92	96	100	104
1	30	150	87	33	13	7	0	3	23	37	57	93
	30	150	63	43	20	13	3	3	17	53	43	93
2	50	150		58	24	8			18	36	68	
	50	150		56	28	14			10	40	72	
3	60	300	87	50	13	7	0	2	20	43	65	92
	60	300	78	48	17	10	2	3	15	50	48	97
4	100	300		56	25	9			22	40	66	
	100	300		56	29	20			11	41	60	
5	90	450	83	49	13	7	0	4	19	48	68	89
	90	450	77	44	20	9	1	3	20	48	53	94
6	150	450		57	29	8			20	43	67	
	150	450		51	28	17			12	49	64	

* The standard stimulus was always 100 grm.

NORMAL INTERPOLATION

The interpolation process used may be called normal inasmuch as it is based upon the assumption of normally distributed data. It is a simplified procedure for calculating

Fernberger and of Rudisill, with certain of whose results the present were compared, were set at 92 strokes per minute while the writer under his conditions was forced to be content with 82. In accordance with the research of Garrett slightly lower precision should accompany this slower beat. *Cf., Arch. Psychol.*, 1922, 56, 24 *ff.*

the threshold from relative frequencies obtained by the method of constant stimuli. If two of the stimuli be selected as a basis for the interpolation, the two corresponding frequencies of report make possible the determination with integral tables of two corresponding distances from the limen in terms of some measure of dispersion such as gamma or usually more conveniently, sigma. Thus knowing the relative position of the limen in terms of dispersion it becomes possible to ascertain its value in terms of stimulus scale. The calculation has been discussed elsewhere and it is performed by substitution in the formula:[7]

$$R_{50} = R_L + \frac{V_L}{V_L + V_U}(R_U - R_L). \qquad (1)$$

Wherein, R_{50} = limen or median,
R_L = a lower stimulus value,
R_U = an upper stimulus value,
V_L = the sigma distance between the frequencies corresponding to R_L and R_{50},
V_U = the sigma distance between the frequencies corresponding to R_U and R_{50}.

This formula is applicable when R_L is less than and R_U greater than R_{50}. When R_L and R_U are both less than R_{50} extrapolation is necessary and the formula becomes:

$$R_{50} = R_L + \frac{V_L}{V_L - V_U}(R_U - R_L). \qquad (2)$$

There is obviously a third case in which R_L and R_U are both greater than R_{50} and extrapolation is again required:

$$R_{50} = R_L - \frac{V_L}{V_U - V_L}(R_U - R_L). \qquad (3)$$

All three formulæ are applicable without modification to frequencies of judgments in the category either of 'greater' or 'less.' Furthermore, their application is not limited to a single pair of stimuli and the respective frequencies; for R_L may be made an average of lower values and R_U of upper

[7] *Op. cit.*, 393–5.

values. Though the chief value of the equations is supposed to be technical, it is of some interest theoretically that there is a simple derivation for each of them.[8] These several facts

[8] Case 1.

$$\frac{R_{50} - R_L}{\sigma} = V_L,$$

$$R_{50} - R_L = V_L\sigma.$$

Similarly

$$R_U - R_{50} = V_U\sigma.$$

Adding

$$R_U - R_L = \sigma(V_U + V_L);$$

$\therefore$

$$\sigma = \frac{R_U - R_L}{V_U + V_L}.$$

But

$$R_{50} - R_L = V_L \frac{R_U - R_L}{V_U + V_L};$$

$\therefore$

$$R_{50} = R_L + \frac{V_L}{V_U + V_L} R_U - R_L.$$

Case 2.

$$\frac{R_{50} - R_L}{\sigma} = V_L,$$

$$R_{50} - R_L = V_L\sigma.$$

Similarly

$$R_{50} - R_U = V_U\sigma.$$

Subtracting

$$R_U - R_L = \sigma(V_L - V_U);$$

$\therefore$

$$\sigma = \frac{R_U - R_L}{V_L - V_U}.$$

But

$$R_{50} - R_L = V_L \frac{R_U - R_L}{V_L - V_U};$$

$\therefore$

$$R_{50} = R_L + \frac{V_L}{V_L - V_U} R_U - R_L.$$

Case 3.

$$\frac{R_U - R_{50}}{\sigma} = V_U,$$

$$R_U - R_{50} = V_U\sigma.$$

Similarly

$$R_L - R_{50} = V_L\sigma.$$

Subtracting

$$R_U - R_L = \sigma(V_U - V_L);$$

$\therefore$

$$\sigma = \frac{R_U - R_L}{V_U - V_L}.$$

become more evident if considered in connection with the theoretical psychometric functions.

The selection of the stimulus values which shall serve as the basis for interpolation (or extrapolation) is a matter which merits some attention. When five comparison stimuli are used experimentally it would be wasteful of the data to use only two of these values and the corresponding frequencies in interpolating. Therefore four were used, and as in the previous study, the average of the two lower of these stimuli was taken for R_L and of the two upper for R_U. The corresponding frequencies of report, averaged in the same way, made possible the determining with integral tables, of corresponding values of V_L and V_U.[9] The question quite naturally arose in connection with the selection of stimuli as to which of the four out of the possible five stimuli should be used. Frequencies of .00 or 1.00 were never used for obvious reasons, but apart from that the rule followed in this and the previous study was 'to select that set of two adjacent pairs of frequencies the average of whose averages would be nearest 0.50.' The stimuli were thus selected on the basis of the frequencies obtained by using them experimentally. The assumption underlying the rule is that the nearer values lie to the limen the more accurate the interpolated value will be. The empirical test of this assumption might form the subject of a separate study. In the experiments with but three comparison stimuli, one of the stimuli was selected to represent R_L and another R_U. The principle of selection was here much as heretofore, *i.e.*, those two adjacent values were chosen whose frequencies averaged closest to 0.50.

The interpolation procedures described above as well as the constant process were applied to sets of relative frequencies

But

$$R_L - R_{50} = V_L \frac{R_U - R_L}{V_U - V_L};$$

$\therefore$

$$R_{50} = R_L - \frac{V_L}{V_U - V_L} R_U - R_L.$$

[9] For the short ranges involved, this simple averaging process assumes a rectilinear relationship between magnitude of stimulus and frequency of report. *Cf.*, Brown and Thomson, 'Essentials of Mental Measurement,' 2d edition, 62.

resulting from the weight-lifting experiment. To the comparison of the limens calculated by these different processes we may now turn our attention.

Approximation of Interpolated to Constant Process Limens

A standard of comparison is required to estimate the significance of the difference between a constant process limen, L, and the limen interpolated on the basis of the same data, R_{50}. The chosen standard was the difference between two constant process limens based on different observed frequencies in the same category of judgment and from temporally mixed series of the same subject. It is difficult to see how such a difference could be other than statistically accidental or unreliable. If, therefore, a comparison between this difference, $L' - L''$, and the difference between either L and the interpolated limen from the same data as of that L, that is, D of R_{50}, shows them to be of the same order of magnitude, then the latter difference is relatively insignificant. Table II., columns 4, 6 and 8, contains the values obtained by the subtraction from D of R_{50} of $L' - L''$. In 77 per cent of the 48 instances these values are negative. This means that 77 per cent of the departures of the interpolated limens from the C.P.L.s [10] are actually less than the departures of the two C.P.L.s from each other; if the latter are insignificant, the former would seem to be so.

The probable error of the difference between L' and L'' was calculated and found to be greater than $L' - L''$ in 20 out of the 24 instances.[11] If, therefore, instead of the differences D

[10] Both L and C.P.L. are used to indicate constant process limen.

[11] The formula for the P.E. used in these calculations is $\text{P.E.}_{\text{C.P.L.}} = .84535 \frac{1}{h\sqrt{2n}}$.

Even though the $\text{P.E.}_{\text{diff.}}$ was in every case except 4 larger than the observed difference, the two sets of values are really of about the same order of magnitude. Excluding one very extreme case, the range for $L' - L''$ is .06 – 2.70 while the range for $\text{P.E.}_{\text{diff.}}$ is .65 – 2.42. In view of the considerable doubt and criticism concerning the meaning and employment of the P.E. in general and of the $\text{P.E.}_{\text{C.P.L.}}$ in particular, these similarities are not without theoretical interest. The discovery of essential agreement between the $\text{P.E.}_{\text{diffs.}}$ and the observed differences, even in these limited data, constitutes some empirical justification of the P.E. formulæ involved, as well as of the observed difference as our comparison standard.

of $R_{50} - (L' - L'')$ the differences D of $R_{50} - \text{P.E.}_{\text{diff.}}$ had been computed, even smaller values than those in Table II. would have been obtained. This fact tends to justify the use of $L' - L''$ as a conservative standard of comparison even though the precision of the data is relatively low as already mentioned. The same fact, therefore, tends to confirm the view that the departures of the interpolated limens from the constant process are insignificant.

TABLE II

DIFFERENCES BETWEEN CONSTANT PROCESS LIMENS, L' AND L'', OF THE SAME SUBJECT IN THE SAME SESSION, COMPARED WITH CORRESPONDING DEPARTURES OF INTERPOLATED LIMENS

Number of series...		30		60		90	
Number of judgments.....		150		300		450	
Subject Number	Judgment Category	$L' - L''$	D of $R_{50} - (L' - L'')$	$L' - L''$	D of $R_{50} - (L' - L'')$	$L' - L''$	D of $R_{50} - (L' - L'')$
1	L	1.18	− .85	.41	− .35	.33	− .30
			− 1.05		− .41		− .29
	H	.26	− .17	.56	− .42	.45	− .29
			− .02		− .28		− .21
2	L	4.37	− 4.17	.07	.20	.06	.26
			− 4.19		.26		.08
	H	1.53	− .87	2.70	− 1.84	.71	.19
			− .30		− .50		.48
3	L	.63	− .62	.41	− .40	.82	− .75
			− .46		− .30		− .65
	H	.28	.57	.71	.11	.31	− .25
			1.11		− .71		− .30
4	L	.60	− .51	.54	− .04	.10	.03
			− .59		− .36		.10
	H	.06	− .04	1.14	− .31	.40	− .31
			− .04		− .97		− .18
Mean of $L' - L''$		1.11		.82		.40	
Mean of D of $R_{50} - (L' - L'')$			− .87		− .43		− .22
Per cent D of R_{50} less than $L' - L''$			88		81		63
Per cent R_{50} less than L		63		44		50	

Since Table II. contains nothing but calculated or derived data it may be made more comprehensible by detailed description. It is based on the relative frequencies from the 4

subjects who lifted 5 comparison stimuli. The data were fractionated at 30 and 60 series as indicated in the top horizontal row. The second row gives the number of judgments, a figure which can be obtained by multiplying together the number of series and the number of stimuli. The first two columns give respectively the subject designation and the category of judgment, *i.e.*, *L* (lighter) or *H* (heavier). All figures in the body of the table represent grams. Column 3 (also 5 and 7) contains the difference between two strictly comparable C.P.L.s., based on temporally mixed series. For instance, the difference between the two 'lighter' limens of Subject 1, based on 30 series each, is 1.18 grm. As already stated, column 4 (also 6 and 8) contains the values of the difference between the interpolated and corresponding C.P.L., *D* of R_{50}, minus the difference between the two comparable C.P.L.s, $L' - L''$. The $-.85$ at the top of column 4 means that *D* of R_{50} is .85 grm. *less* than $L' - L''$; and since $L' - L''$ is in this case 1.18, the value of *D* of R_{50} itself would be .33 grm. The second figure in column 4 refers to the departure of the other interpolated limen from the other C.P.L. in that pair. Since there is only one difference between every two C.P.L.s there is only one figure in column 3 (also 5 and 7) for every two in column 4 (also 6 and 8). The next to the bottom row gives the relative frequency with which *D* of R_{50} is less than $L' - L''$.

The next to the bottom row, a comparison of columns 4, 6 and 8, and the means of those columns all show the same thing, *i.e.*, a tendency for the values to increase with the number of series. There are two reasons for this tendency, first, the greater the number of series the less the difference between L' and L'', a fact disclosed in columns 3, 5 and 7 or in the means of those columns, and to be expected on the basis of the theory of the probable error. In the second place, the *D* of R_{50} shows a tendency to be independent of the number of series, a fact which conforms to expectation from the nature of the computation. With the tendency of *D* of R_{50} to remain constant and of $L' - L''$ to fall as the number of series increases, the values in the table necessarily increase.

The 'per cent D of R_{50} less than $L' - L'''$' at the bottom of the last column of Table II. is 63 and may be compared with 82, the corresponding figure in Table V. of the prior investigation already referred to several times. This disparity is directly ascribable to the larger values of $L' - L''$ in the Table V. It is highly probable that these values of $L' - L''$ are the larger because the relative frequencies from which they were computed were not obtained simultaneously, or to be exact in mixed series, as was the case in the present study. The $L' - L''$ values herein constitute the stricter standard of comparison even despite the somewhat lower statistical precision of the limens.

The bottom row of the table shows some tendency for the interpolated limens to be as often as not less in value than the C.P.L.s.

Consistency of Interpolated Limens

The constant process limen itself has been shown to be not above criticism as a standard of exactitude.[12] For this reason in part, and also because of the desirability of acquiring data on the reliability of interpolated limens, various interpolated limens have been compared with each other by product moment correlation. Each two limens correlated were based on the same number of judgments in the same category at the same time, that is, in mixed series, by the same subject. Table III. shows the results of the correlations. Column 1 gives the numbers of judgments represented by the limens, column 2 the number of cases or pairs of limens correlated, and column 3 the correlation coefficients. In general, these high coefficients indicate close relationship or small amount of difference between the interpolated limens. The correlations between these interpolated limens calculated from different sets of relative frequencies are, indeed, as high as were correlations between interpolated and C.P.L.s both calculated from the *same* set of relative frequencies.[13]

The last three columns in the table are of particular interest. The 5th and 6th show the probable errors of

[12] Culler, E., Psychol. Monog., 1926, 35, No. 2, 80–4.

[13] *Op. cit.*, 396.

estimate of one interpolated limen, given another. For instance, the .42 at the top of the 5th column indicates that an R_{50}' limen estimated from an R_{50}'' limen will most probably depart .42 grm. from its true value. The probable errors of

TABLE III

CORRELATIONS BETWEEN COMPARABLE INTERPOLATED LIMENS [14]

Judgments	Cases	r	$\sqrt{1-r^2}$	P.E.$_{est}$ R_{50}'	P.E.$_{est}$ R_{50}''
150	24	.95	.31	.42	.52
300	23	.94	.34	.43	.54
450	24	.995	.10	.13	.15

estimate of the limens representing 450 judgments, the table shows to be only .13 and .15 grm. As compared with this an estimation error of .33 grm. was found for C.P.L.s from interpolated limens when both were calculated from the *same* set of relative frequencies and 500 judgments.[15] The 4th column gives the 'coefficients of alienation.[16] These are intended to indicate the extent to which the correlation reduces the error of estimate as compared with a mere guess. The correlation of .995 for example, reduces the error of estimate to 0.1 of the error of a random estimate. On the whole the results show a relatively high order of consistency among interpolated limens; the latter appear to agree with each other at least as well as with a constant process calculation from the same set of frequencies.

The important question of a suitable measure of reliability remains unanswered but certain suggestions present themselves. The calculated probable errors of the differences of the C.P.L.s were found to be of about the same order of magnitude as the observed differences themselves.[17] These

[14] It must be noted that the second correlation plot contained a single very extreme deviation from the straight line of best fit. In computing the coefficient this extreme case was thrown out because both its obvious nonconformity and the fact that neither of the other two correlations contain extreme deviations, suggest that it should be ascribed to defective procedure. The correlation including the extreme case would be .89.

[15] *Op. cit.*, 396.

[16] Kelly, T. L., 'Statistical Method,' 173.

[17] *Cf.* footnote 1, p. 53.

observed differences between the C.P.L.s furthermore were of the same order as the observed differences between the interpolated limens.[18] There were thus empirical indications that the P.E. formulæ not only tend to represent the facts in respect of C.P.L. variability but might perhaps also be adapted to measure the unreliability of the interpolated limen.

The most recent formula for the probable error of the constant process limen is as follows:[19]

$$\text{P.E.}_{CPL} = .84535 \frac{1}{h\sqrt{2n}}.$$

Here .84535 is the multiplying constant for converting the standard error into the probable error of the median, n is the number of series, and it has been shown that $\frac{1}{h\sqrt{2}}$ equals sigma.

$$\text{Hence P.E.}_{CPL} = .84535 \frac{\sigma}{\sqrt{n}}.$$

To adapt this formula to the interpolated limen it would appear only necessary to provide the calculation of sigma. Now a value for sigma in a normal distribution readily could be obtained by interpolating or extrapolating the stimulus values corresponding to $p = .1587$ and $p = .8413$ and then taking half their difference. Since the interpolations would be carried out independently of these p-values, the number of decimal places in which the latter are expressed would not affect the accuracy of the calculation.

$$\sigma = \frac{R_{.8413} - R_{.1587}}{2}.$$

The P.E. formula would then become:

$$\text{P.E.}_{R_{50}} = .84535 \frac{R_{.8413} - R_{.1587}}{2\sqrt{n}}.$$

Statistical logic would demand that the calculations for the measures of both central tendency and variability be based

[18] *Cf.* Tables II. and IV., columns 3, 5 and 7.

[19] Culler, E., PSYCHOL. MONOG., 1926, 35, No. 2, 85–90. [See Note 25.]

upon the same data. Therefore, the identical set of stimulus values, R_L and R_U, and corresponding frequencies, should be used both in calculating the limen and in interpolating from .1587 and .8413. These two latter interpolations could be simply achieved by the use of appropriate formulæ. Two proper formulæ could be selected from a group of nine,[20] all of which are slight modifications of the liminal formulæ whose derivations are given elsewhere.[21]

It seems well to note here that the empirical results on reliability only apply directly to limens secured simultaneously, that is, based on temporally mixed series; only the observed differences between such limens, and in limited number at that, have been demonstrated in approximate correspondence with the calculated probable errors of the differences. Greater variation is to be expected among limens based on experimentation conducted at different times of day or on different days. The comparison between Table II. above and Table V. of the other study shows this.

[20] Case 1. When R_L is less than R_{50} and R_U is greater.

$$\text{If } R_L < R_{.1587}, \text{ then } R_{.1587} = R_L + \frac{V_L - 1}{V_L + V_U} R_U - R_L.$$

$$\text{If } R_L > R_{.1587}, \text{ then } R_{.1587} = R_L - \frac{1 - V_L}{V_L + V_U} R_U - R_L.$$

$$R_{.8413} = R_L + \frac{V_L + 1}{V_L + V_U} R_U - R_L.$$

Case 2. When R_L and R_U are both less than R_{50}.

$$\text{If } R_L < R_{.1587} \text{ then } R_{.1587} = R_L + \frac{V_L - 1}{V_L - V_U} R_U - R_L.$$

$$\text{If } R_L > R_{.1587} \text{ then } R_{.1587} = R_L - \frac{1 - V_L}{V_L - V_U} R_U - R_L.$$

$$R_{.8413} = R_L + \frac{V_L + 1}{V_L - V_U} R_U - R_L.$$

Case 3. When R_L and R_U are both greater than R_{50},

$$R_{.1587} = R_L - \frac{V_L + 1}{V_U - V_L} R_U - R_L$$

$$\text{If } R_L < R_{.8413}, \text{ then } R_{.8413} = R_L + \frac{1 - V_L}{V_U - V_L} R_U - R_L.$$

$$\text{If } R_L > R_{.8413}, \text{ then } R_{.8413} = R_L - \frac{V_L - 1}{V_U - V_L} R_U - R_L.$$

[21] Footnote 8.

Number of Comparison Stimuli

There are several reasons why it is important to know whether or not the number of the comparison stimuli can be reduced to as few as two or three. When the method of constant stimuli is used with five stimuli, say, one or two of the extreme relative frequencies occasionally comes out .00 or 1.00 and therefore cannot be used in calculating the limen. There are only two courses open, either repeat the experimentation after appropriate change in the stimuli or else compute the limen on the basis of the reduced frequencies. The latter, easier procedure would receive some justification if three stimuli were demonstrated to be substitutive for five. In the second place, the task of selecting appropriate stimuli for the experimentation becomes easier as their number is reduced. Were seven required, for instance, the chances of selecting inappropriate stimuli, especially values which would give .00 or 1.00 relative frequency would be great; where only three are required it is comparatively easy to select values within the range of the psychometric function. The preliminary experimentation can obviously be reduced. Again, it is quicker to experiment with fewer stimuli; it is also less expensive, a matter of no small moment when elaborate construction is involved as for instance in Culler's thermal investigation.[22] Finally, if 2 stimuli were found to be adequate the application of the normal interpolation procedure would be simplified to the extent that no averaging of relative frequencies would be necessary.

There are several psychological difficulties to contend with in reducing the stimuli.[23] The fewer the stimuli the less the probability of 'procedure without knowledge.' The fewer the stimuli the more chance of the subject's learning an order of presentation or of securing a secondary criterion. These as well as a tendency to guessing or perfunctory judgment are, moreover, enhanced by a knowledge that the number of stimuli is very small. In the present experiment this diffi-

[22] *Arch. of Psychol.*, 1926, 81, 15–21.

[23] There is also the technical objection that the fewer the comparison stimuli are the less satisfactory the graphic check of the ogival relationship between stimulus and frequency becomes.

culty appears to have been met satisfactorily by allowing the subject to believe that the maximum number of comparison weights, *i.e.*, five, was being used in all of the experimentation. Nothing of this was said to the subject but the experimenter always kept the full set of the weights on the turntable used for presentation. If the subject, therefore, should happen to glance at it when leaving or entering the room he would be likely to get a suggestion of no change in the number of weights. During the introspections, taken at the end of the experiment, each subject was asked whether he had noticed 'any difference in the number of weights used on different days.' All except one replied that they had not and two added that they had not even considered the possibility. The subject who reported perception of difference said he could not tell on which days it occurred. The data of this subject do not appear to differ essentially from those of the others.

In this study 5 comparison stimuli were used in some sessions and the 3 intermediate stimuli of the same set in the other sessions. The dropping of the two *extreme* stimuli provided an opening for another classical objection. The extreme stimuli are supposed to have a steadying effect upon the subject and to therefore exert a favorable influence upon the judgments of those stimuli and especially on the others which more closely approximate the threshold. During his introspective session each subject was asked whether he observed 'any difference in surety or confidence on different days.' All subjects reported fluctuations but they do not appear to have coincided with the number of stimuli employed, that is to say, the records show no tendency to less confidence on the days when the extreme stimuli were omitted. And the two subjects who never lifted the extreme stimuli in the experimental sessions appear to have been as confident as the others. It may be that these introspective reports on confidence are of little value, however, for they were never made until the subject was through the experiment and it may well have been too much to ask him to retrospect over a period of a week. That there was no very marked effect upon confidence seems reasonably certain.

We should not have been able to meet difficulties of the character mentioned above by following either of Fernberger's types of procedure.[24] In his theoretical study he applied the constant process to the observed frequencies from all seven comparison stimuli and did likewise after dropping the two extreme frequencies; the limens from these extended and artificially reduced series were then computed and compared. In the experimental study the 'classical series of seven' was mixed with a reduced series of five; thus the data for both limens were again obtained at the same time. In both procedures, therefore, the subject was in a position to receive in the reduced series any steadying or other favorable influence the longer series might confer by reason of simultaneity. While 5 stimuli have been shown to be adequate even when used alone, it is not at all clear that 3 would prove to be so. For that reason the present experimentation with 3 stimuli was executed in different sessions from that with the 5. As a control on the possibility of some steadying or other effect of the 5 stimuli carrying over to a subsequent 3 stimuli session, there were the two subjects who lifted nothing but 3 stimuli throughout.

The purpose of Tables IV. and V. is to show how the limens based on experimentation with 3 stimuli compare in variability and in value with those based on experimentation with 5 stimuli. The arrangement, with two exceptions, is the same as in Table II. above. The standard of comparison or subtrahend is now the difference between two interpolated limens (obtained at the same time, etc.) instead of as in Table II. the difference between two C.P.L.s (obtained at the same time, etc.). The other point of difference between the tables lies in the minuends of the subtractions whose results appear in columns 4, 6 and 8. Whereas in Table II. the minuend was the departure of the interpolated from the C.P.L. of the same frequencies, here it is simply the difference between two interpolated limens (obtained at the same time, etc.). Each difference given in Table IV. is, then, the difference between two comparable limens based on 3 stimuli minus the difference

[24] *Amer. J. Psychol.*, 1914, 25, 121–130 and PSYCHOL. REV., 1914, 21, 335–355.

between two comparable limens based on 5 stimuli. The latter limens were invariably obtained on the following experimental day. Table V. is comparable to Table IV. in that it is identical in character except that the subtrahends as well as the minuends represent experimentation with only 3 comparison stimuli.

TABLE IV

COMPARISON OF THE DIFFERENCES BETWEEN COMPARABLE INTERPOLATED LIMENS BASED ON EXPERIMENTATION WITH THREE COMPARISON STIMULI, WITH THE DIFFERENCES BETWEEN OTHER SUCH LIMENS BASED ON EXPERIMENTATION WITH FIVE COMPARISON STIMULI

Number of Judgments		150		300		450	
Subject Number	Judgment Category	$R_5' - R_5''$	$(R_3' - R_3'') - (R_5' - R_5'')$	$R_5' - R_5''$	$(R_3' - R_3'') - (R_5' - R_5'')$	$R_5' - R_5''$	$(R_3' - R_3'') - (R_5' - R_5'')$
1	*L*	.71	− .64	.34	− .21	.34	.47
	H	.11	.42	.42	− .05	.36	.52
2	*L*	3.98	− 2.28	.52	− .06	.41	− .16
	H	2.10	2.04	4.04	2.19	1.00	1.57
3	*L*	.81	− .13	.53	.41	.91	− .25
	H	.81	.02	.11	.79	.38	.41
4	*L*	.51	− .36	.22	.69	.17	.01
	H	.07	.71	2.14	− 2.10	.52	− .42
Mean of $R_5' - R_5''$		1.14		1.04		.51	
Mean of $(R_3' - R_3'') - (R_5' - R_5'')$			− .03		.21		.26
Per cent $R_3' - R_3''$ less than $R_5' - R_5''$			50		50		38
Per cent R_3 less than R_5		31		25		31	

Comparison of the negative with the positive differences in Table IV., columns 4, 6 and 8, shows that the difference between the R_3 limens is about as often as not less than that between the R_5; the next to the last horizontal row summarizes this relationship. The third from the bottom row shows that the means of these differences are not far from zero. There is, therefore, little indication of increased variability for limens based on reduced frequencies, a result which is supported by the fact that the differences appear quite inde-

TABLE V

COMPARISON OF DIFFERENCES BETWEEN COMPARABLE INTERPOLATED LIMENS BASED ON EXPERIMENTATION WITH THREE COMPARISON STIMULI, WITH THE DIFFERENCES BETWEEN OTHER SUCH LIMENS BASED ON THE SAME NUMBER OF COMPARISON STIMULI

Number of Judgments.....		150		300		450	
Subject Number	Judgment Category	$R_3' - R_3''$	$(R_3' - R_3'') - (R_3' - R_3'')$	$R_3' - R_3''$	$(R_3' - R_3'') - (R_3' - R_3'')$	$R_3' - R_3''$	$(R_3' - R_3'') - (R_3' - R_3'')$
5	L	1.29	− .76	1.07	− .60	.62	− .37
	H	.01	.67	.59	− .09	.35	.03
6	L	1.87	− 1.44	2.30	− 1.88	1.23	− .53
	H	3.09	− .55	.33	5.35	.26	4.52
Mean of $R_3' - R_3''$		1.57		1.07		.62	
Mean of $(R_3' - R_3'') - (R_{-3}' - R_{-3}'')$			− .52		.70		.91
Per cent $R_3' - R_3''$ less than $R_3' - R_3''$			75		75		50
Per cent R_3 less than R_3		50		50		50	

pendent of the number of judgments or the size of the comparison standard, $R_5' - R_5''$. Table V. compares, in limited data to be sure, the difference between comparable R_3 limens obtained at one time with the difference between others obtained at another time; and the one difference appears to be less than the other about half of the time. If, then, Tables IV. and V. now be considered together there is some limited evidence that the variability of simultaneously secured R_3 limens is no greater in comparison to that of simultaneously secured R_5 limens than it is in comparison to a second set of the R_3 limens. In summary, the variability of an R_3 limen obtained at one time appears to be of about the same order as the variability of either another R_3 or an R_5 limen obtained a day or two later. Furthermore, a comparison of Table IV. with Table II., columns 3, 5 and 7, shows the values of $R_5' - R_5''$ to be of the same order as the values of $L' - L''$. The facts indicate that the variability of interpolated limens is about the same as that of C.P.L.s.

Above mentioned results tend to show that the R_3 variability is of a magnitude comparable to the R_5; but they do not tell whether the values of the limens are comparable. The R_3 limens might tend to run constantly smaller, for instance, than the R_5 limens; though on rather general principles we should expect no such constant difference; rather a balancing of the positive and negative divergencies, or in other words similar values for the two kinds of limens. Data bearing on this point have been summarized in the bottom rows of the tables. The bottom row in Table IV. gives the relative frequencies with which the R_3 limens were less than the corresponding R_5 limens; each figure is based on 16 cases. There appears to be a tendency for the R_3 limens to run somewhat higher than the R_5 which there is some reason for regarding as an artifact of limited data. Still the corresponding values in Table V., based on but 8 cases each, indicate that R_3 limens secured at one time are as often as not less than those secured in the next session. We cannot conclude from the present data, therefore, that the value of an R_3 limen secured at one time agrees as well with an R_5 limen secured at another time as it does with a second R_3 limen secured at another time. Both theory and our results do seem to indicate, however, that the limens based on experimentation with 3 comparison stimuli *approximate* those based on 5 stimuli in value as well as in variability.

Summary

The interpretations of the limited data of this investigation receive support from its agreement with a previous study along similar lines. The principal results are as follows:

1. Limens calculated by the interpolation process described do not appear to differ essentially in value from limens calculated by the constant process from the same data.

2. The interpolated limens appear to be of a fairly satisfactory order of reliability, one which in fact appears about the same as that of the constant process.

3. A measure of reliability for the interpolated limen is suggested and to some extent empirically justified.

4. It seems technically practicable to reduce the number of the comparison stimuli used in the experimentation from 5 to 3.

5. Limens calculated by the normal interpolation from the reduced number of frequencies appear to remain of about the same order of value and of variability as when 5 stimuli are used, at least when the subject passes the same total number of judgments.

The above results as well as the simplicity of the interpolation process point to its feasibility as a procedure for general use.[25]

[25] Since this article was written, Culler has pointed out (J. EXPER. PSYCHOL., 1927, 10, 475 et supra) that when 5 or more comparison stimuli are used, a certain modification of his formula for the P. E. of the C. P. L. given above (p. 58) is desirable.

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ABILITY VERSUS EFFECTIVE ABILITY

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In a preceding article it was shown that certain modern writers seem to assume that mere possession of ability is a guarantee of ultimate efficiency (1). This opinion has been set forth from time to time in the literature of the past. Galton and Schuster make the following comment in this regard:

> The demand for exceptional ability when combined with energy and good character is so great that a lad who is gifted with them is hardly more likely to remain overlooked than a bird's nest in the playground of a school (2).

The genius is assumed to have as a correlate to the unusual ability impelling drives which culminate in unusual performance. This motivation is thought to be inherent in the ability, an almost invariable and inevitable correlate to the ability. Woodworth has taken recently a somewhat similar position in this regard, asserting that inordinate capacity carries with it the ability to seek successfully the opportunity which will permit the ability to express itself (3).

William James long ago took exception to this assumption, remarking that the best wood-pile will not blaze unless a torch is applied (4). If a wood-pile blazes it is unreasonable to attribute that fact to any one integer, for example, the condition of the wood, the quality of the wood, or any other single agent. Wood, torch, suitable weather conditions, etc., all are essential to the blaze. Obviously, the analogy is applicable to a discussion of genius. Probably men are equipped by nature with varying amounts of ability for various types of work. Seashore has shown that a small percentage of the population is relatively unresponsive to differences in pitch (5). Environmental provision seems to

have little ameliorating effect upon the disability (5). It would therefore be foolhardy to provide unusual opportunity for such individuals in the field of music. It seems probable that outstanding potential ability must be present in the individual who makes original and noteworthy contributions in any field. However, ability alone does not guarantee accomplishment. If the potentially able individual lacks drive, or desire to accomplish, he will not achieve noteworthy success in life.

Numerous writers have asserted that it is intensive effort only that educates. Intensity of effort during the initial periods of endeavor usually is accompanied by dissatisfyingness and nature provides no natural tendency toward prolonged intense effort. The individual who makes conspicuous contributions to human goods therefore must be actuated and dominated by impelling motives which furnish the *impetus* to sustained effort. This is particularly essential in the initial periods of developing a skill. Later, the activity itself may furnish the stimulus to further activity but the initial periods are usually attended by dissatisfyingness which thwarts further activity unless impelling drives are present. Ability and drive therefore are necessary correlates to effective genius.[1]

[1] William James repeatedly emphasized this fact. In his 'Talks to Teachers' he remarked as follows: "The total mental efficiency of a man is the resultant of the working together of all his faculties. He is too complex a being for any one of them to have the casting vote. If any one of them do have the casting vote, it is more likely to be the strength of his desire and passion, the strength of the interest he takes in what is proposed. Concentration, memory, reasoning power, inventiveness, excellence of the senses, . . . all are subsidiary to this. No matter how scatter-brained the type of a man's successive fields of consciousness may be, if he really *care* for a subject, he will return to it incessantly from his incessant wanderings, and first and last do more with it, and get more results from it, than another person whose attention may be more continuous during a given interval, but whose passion for the subject is of a more languid and less permanent sort. Some of the efficient workers I know are of the ultra-scatterbrained type. . . .

"This preponderance of interest, of passion, in determining the results of a human being's working life, obtains throughout. No elementary measurement, capable of being performed in a laboratory, can throw any light on the actual efficiency of the subject; for the vital thing about him, his emotional and moral energy and doggedness can be measured by no single experiment, and becomes known only by the total results in the long run. . . .

It is frequently said that genius is merely infinite capacity for taking pains, for doing an enormous amount of work. The writers do not agree with this concept of genius. Thousands of lace-makers, weavers, road-workers, laborers of every description have demonstrated the ability to take infinite pains but in the history of the world very few such persons have produced works of genius. It is therefore of doubtful validity to argue that genius is solely the fruit of patient endeavor. It is the belief of the present writers that the fruits of genius are a function of no less than three integers, namely, *ability, drive, and opportunity.* It is their belief that ability does not become *effective ability* apart from drive and opportunity. This assertion is so obviously true that it would not require elaboration but for the tendency on the part of some recent writers to over-simplify the problem of genius.

In the present paper the possible origins of that drive which seems to characterize the effective genius will be commented upon briefly. It is clear that if the genius is driven there must be one or more reasons for that fact. Drive is not something that merely happens to exist without cause. Nor has drive been proven to be inherent in superior capacity. Drive is doubtless due to certain antecedent facts though these causes are sometimes so subtle and unobtrusive that they often elude identification.

The Nature of Drive

The present writers have been employing the term drive without definition. By drive, they mean any tendency or disposition, implicit or explicit, toward intense or persistent activity. This tendency or disposition to act may or may not be evident to the one who is driven. If the tendency to act be undetected by the individual, it may be described as an

"Be patient, then, and sympathetic with the type of mind that cuts a poor figure in examinations. It may, in the long examination which life sets, come out in the end in better shape than the glib and ready reproducer, its passions being deeper, its purposes more worthy, its combining power less commonplace, and its total mental output consequently more important." James, 'Talks to Teachers,' New York, Henry Holt & Co., 1899, pp. 114, 134–7, 142–3. Quoted by Skinner, C. E., Gast, I. M., and Skinner, H. C., 'Readings in Educational Psychology,' New York, D. Appleton & Co. 1926, pp. xxvii–833 (pp. 683–4).

'unconscious' or a 'subconscious' drive. The resultant activity may be a direct gratification either of instinctive or of acquired desire. Also, the resultant activity may be indirectly gratifying. In this case the behavior may be called 'compensatory' activity. Much compensatory activity results from conflicting impulses which arise in the thwarted organism. E. S. Robinson has asserted that when desires are precluded or inhibited, substitute behavior results which often includes in modified form all the essential elements of the original desires (6). Sometimes, however, the substitute activity contains little or no direct evidence of the specific constituents of the conflict which effects the driving power. However, careful analysis of these cases will reveal frequently that such activity provides indirect gratification for the thwarted impulses. It is at once apparent that the forces that give rise to drive are numerous and varied.

The Origin of Drive. (*a*) Native Endowment

Until recent years, there was a tendency to attribute much of the activity of man to drives occasioned by native endowment. There has been much controversy of late regarding identification and definition of man's original nature. There are three groups of original tendencies designated by Thorndike, namely, reflex, instinct, and capacity (7). These tendencies are thought by him to be the result largely of 'determiners' in the germ cell. They constitute the unlearned equipment of man. The ultimate criterion for the inclusion of any trait in one of these categories rests upon demonstration of its functioning in the absence of opportunity for learning. Obviously, behavior appearing at or immediately following birth is native. The only valid way to ascertain whether other activities are innate or acquired is through genetic study of children through the successive years of growth.

The three groups of original tendencies differ chiefly in complexity and the blanket term instinctive is used to include all. The relatively simple activities, sneezing, swallowing, grasping, etc., are designated reflexes. More complex pattern

reactions, such as walking, crying, fighting, etc., are usually called instincts, while capacity is used to posit indefinite response to very complex situations in which environmental forces are important.

The simplest forms of instinctive activity have certain unmistakable characteristics which Wada's experiments with hunger clearly demonstrate (**8**). The infant displays certain bodily conditions, one invariable symptom being the vigorous contractions of the walls of the upper part of the stomach, which form the usual stimuli for the state described as hunger. Hunger is clearly *impulsive*, there is *readiness* and *desire* for food. The subject becomes restless and tries out a repertoire of responses until one which satisfies is obtained. The reaction resulting in the satisfying state may be called the consummatory reaction. An important characteristic, namely, the dynamic or driving nature of the instinct is herein revealed. This bodily state of readiness and the resultant drive are characteristics of all instincts. Indeed, McDougall has asserted that the native impulses are the *only* forces to thought and action.

> Are, then, these instinctive impulses the only native powers of the human mind to thought and action? . . . Habits are in a sense derived from and secondary to instincts; for in the absence of instincts, no thought and no action could ever be achieved or repeated, and so no habits of thought or action could be formed. Habits are formed only in the service of the instincts. . . . Take away these instinctive dispositions with their powerful impulses, and the organism would become incapable of activity of any kind (**9**).

McDougall, one sees, emphasizes the importance of certain basic instincts as the *only* motive forces for individual and social activity. The dissatisfaction with this hypothesis is clearly demonstrated through the abundant literature that has resulted in opposition to it.

There are no less than three groups of individuals who exemplify the various attitudes existent regarding instincts: (*a*) Those who deny all instincts as commonly designated;

(*b*) those who admit a small nucleus of unlearned behavior, certain simple human tendencies, *i.e.* the tendency of the baby to suck, to cry when a loud noise is made, to support its own weight when suspended by the hands, to grasp near-by objects, etc.; (*c*) those who maintain that the unlearned equipment of man is a highly complex organization comprising many innate tendencies to respond in definite ways to various stimuli and groups of stimuli.

Proponents of the three schools present convincing evidence and arguments for their respective positions and the student is likely to be confused by the conflicting points of view. There is no way of settling at the present time the age-old question regarding the original nature of man. Genetic studies of childhood are few and meagre and their results to no small degree controversial. However, various experiments have demonstrated rather conclusively that many of the behavior manifestations heretofore commonly called instincts are not characteristic of children in the absence of opportunity for learning.

Perusal of the literature with the vast number of reactions named instincts will convince the reader that many of these are merely mythical entities that exist only in the literature. Nevertheless, there appears to be a nucleus of unlearned behavior differing mainly in strength that furnishes drives or impulses similar to hunger. Collectively, these native traits (sex instincts, anger, multiform mental and physical activity, etc.), form basic dynamic forces which guide and determine human conduct.

The Origin of Drive. (*b*) Glandular Secretions

The psychologist must recognize the importance of glandular activity in determining behavior. Little is known regarding the exact rôle of the glands in effecting attitudes and determining the direction of one's energy. Numerous writers assert that certain hormones play an important rôle in the general behavior of human beings. The thyroid is thought to act as a governor for the whole body; over-secretion of this gland speeds up and under-secretion slows down the action of

the entire body. Thus, the desire to exert extreme effort, the very purposiveness of certain forms of behavior, may owe its origin in part at least to the physiological condition of the organism resulting from hormone activity (**10**).

Other glands too are thought to be potent determiners of behavior.

> It is now believed that the parathyroids give rise to a *secretion which tends to keep in check or restrain overactivity of the nervous system* (*restrains the discharge of nerve cells*) (**11**).

Although the glandular products appear to be associated intimately with the activity of the nervous system, the precise effect of the various glands upon mental attitudes has not been determined. Obviously, the problem is not one that may be investigated easily. It is quite possible that a proper admixture of various secretions is essential to normal, smooth-running bodily endeavor. A disproportionate amount of one or another of the secretions may be one factor in producing intensity of effort or drive.

Nor have the causes of glandular activity been ascertained. It seems possible that glandular function may be the result either of heredity or of environmental factors. The writers mention glandular secretions merely as one of the possible determiners of drive.

The Origin of Drive. (*c*) Habits

The question arises whether other factors than instincts, glandular secretions, etc., may not also furnish powerful drives to behavior. Gates believes that certain habits not derived from instincts are potent determiners of human activity. He calls attention to certain habits which can be directly aroused and continue in action without detectible actuation from hunger, sex, curiosity, or any other instinctive drives (**28**). These habits when actuated seem to furnish their own drives toward consummatory reaction and, once set up, are themselves strong drives to activity. Acquired habits, once set up, seem to operate as mechanisms determining behavior in precisely the same manner as the instincts and

display the fundamental characteristics of drives. A bodily stimulus actuates a state of readiness, preparatory reactions follow, accompanied by a conscious impulse toward consummatory reaction through which satisfyingness may be secured.

Additional Factors which May Cause Drive

Instincts, habits, and perhaps glandular secretions, drive man tenaciously to seek out satisfyingness. But life abounds with obstacles to one's desires. Constant thwarting is unavoidable in human contacts. When one's desires are thwarted the nature of the drive is conspicuously changed. The thwarting of a drive thus creates new drives. Indeed, some writers assert that the thwarting precedes most activity.

> A recent school of biology and biological psychology sets out from the conception of a stationary physiological state, which the organism strives to maintain, or regain if it is disturbed. The designation 'stationary physiological state' is perhaps unfortunate, for in life there is no stationary state. But the fact so designated is nevertheless an obvious one. There is a condition of equilibrium to which the organism constantly tends to return. This equilibrium is not a mechanical, static equilibrium, but a vital dynamic one. Every threatened or actual disturbance of the equilibrium evokes forces and activities which tend to maintain or to restore it (12).

Thus it seems that much human behavior is in reality substitute activity occasioned by the thwarting of instincts or desires. The principle of compensatory activity is a familiar one to the student of psychology. When man's impulses are thwarted, he tries in one way or another to secure a satisfactory adjustment. Indeed, the ability to secure indirect satisfaction is a characteristic human trait. Everyday life abounds with examples of various mechanisms of compensation at work. He who fails in an examination attributes his inadequacy to the unfair nature of the questions, he of reticent and timid nature despises more than anything else aggressiveness or pretentiousness, and he who is physically weak lives with the hero of screen or stage the aggressive life

of a robust man. Through projection, one secures satisfaction by attributing his own deficiencies to others, by rationalization one is able to maintain his self-esteem in the face of irrational behavior, identification permits one to substitute for genuine action imaginary achievements by identifying one's self with the conquering or wounded hero. Numerous other types of compensatory adjustment evidence the capacity of the mind to seek out tenaciously satisfying substitute activities.

Individual differences in the ability to withstand the thwarting of wants are great. So too individuals vary tremendously in the types of adjustment effected when thwarting occurs. Some individuals maintain mental calm and poise in the face of the most harassing circumstances and seldom seek satisfaction through substitute gratification; others, however, are actuated by slight deprivations to seek numerous and varied forms of substitute gratification.

The fact of individual differences in reference to ability to withstand thwarting is at once apparent. The present writers have observed certain racial differences in frequency of participation in certain play activities which appear to be compensatory in character. In a recent article the writers displayed data showing that certain negro children play school much more frequently than white children of corresponding chronological ages (**13**). In this article they suggested that negro children play school more often than white children because this activity symbolized to them knowledge, power, and prestige, which they are unable to achieve in the world of actuality.

In another article the writers showed that negro children engage in church and Sunday school activities much more frequently than do white children (**14**). It was pointed out that the church affords the negro many *easily accessible* and intensely satisfying substitute activities. Contact with the white man brings constant and unmistakable signs of inferiority. But religion stressing the doctrine of equality offers the negro a variety of compensatory interests. The white man also finds solace in religion. However, his participation

in religious activities is conspicuously less than that of the negro. It seems likely that routine life with its multitudinous deprivations produces in the negro a greater need for compensatory reactions than in the white man. These data seem to warrant the hypothesis that religion provides the negro satisfying mechanisms of escape, devices by means of which he frees himself from environmental inhibitions and feelings of inferiority.

The reader may feel that the writers have placed undue emphasis upon the concept of 'escape.' However, it might be said that most human activity is actuated by attempts to escape from unpleasant circumstances and to maintain a satisfactory state. In a recent volume this very thesis is presented by Raup who states that the effort to maintain 'complacency' characterizes most human behavior (15). Raup insists that lack of complacency produces a drive to activity and that with the achievement of complacency, the activity ceases.

In an article entitled 'The Stimulus-Response Fallacy in Psychology,' Thurstone has pointed to the fact that dissatisfaction, want, desire, or motivation precede a given stimulus and that without these antecedents a stimulus is ineffective (16). In other words, he maintains that activity arises from a state of dissatisfaction and that it continues until a state of satisfaction has been achieved. With the achievement of satisfaction the activity ceases.

E. S. Robinson places great emphasis upon the rôle of conflicting impulses in effecting activity (17). Robinson goes so far as to assert that all thinking looked at in this light is, in origin at least, a form of compensatory conflict resolution.

> Perhaps the most characteristically compensatory expressions in human life are carried out in thought. Thinking and imagining are the modified and abbreviated expression of impulses toward action and it is doubtful whether thought would ever have appeared without conflict. It is quite possible, of course, that verbal expression, in so far as it is overt, arose to some extent independently of conflict, but I can see no explanation for the rise of implicit linguistic or non-linguistic ideational behavior except in terms of the

modification and abbreviation in conflict of fuller expression. Most of us teach that thinking is favored by complexity and instability (to a degree) of sensory-motor organization, but it is not such a common procedure to show that thinking arises out of this complexity and instability only to the extent to which these characteristics favor conflicts between impulses and the production of thoughts as a considerably modified manifestation of competing impulses. All thinking looked at in this light is, in origin at least, a form of compensatory conflict resolution (17).

One sees clearly therefore the importance of conflicts in determining the direction of one's impulses. Much of life activity seems to result from conflicts occasioned by denial of our desires. These compensatory reactions vary tremendously in nature. Sometimes, they may be the very expressions which the world has designated genius. Krutch asserts that Edgar Allan Poe's genius was largely compensatory reaction by means of which satisfyingness was indirectly secured for certain thwarted physical and social desires. It is interesting to examine the works of genius in the light of the extent to which these works may have resulted from drives set up in the attempt to gratify indirectly certain powerful desires which life thwarts. For example, Jeannette Marks insists that it is a question whether the type of man who is perfectly equipped to enjoy life would write poetry. She suggests that suffering, disease, and disaster produce the will to create (18). Of Thomson's greatest work, Miss Mark writes:

'The City of Dreadful Night' was written during periods of mental suffering but the poet was not friendless. Thomson himself said that it 'was the outcome of a good deal of sleepless hypochondria suffered at various periods' (18), p. 116.

The forces which give rise to substitute activity are numerous and varied. The present writers have commented upon a few of these forces only. To the reader it must be apparent that the *drive* to use the ability one possesses may be wrought by the thwarting of one's strong desires. The striving to obtain substitute gratification is insistent, and

dynamic. The drive in some cases is thus set up and sometimes may result in unusual creation, the work of genius sometimes recognized by the world but always intensely satisfying to the worker.

The Subconscious

Many psychologists believe that much human behavior cannot be accounted for entirely in terms of conscious reaction. Although loath to employ such terms as unconscious or subconscious, psychologists have been unable to ignore certain phenomena to which psychoanalysts have called attention. Acceptance of the view that much of human activity is unconscious does not posit acceptance of Freudianism or acceptance of the numerous claims of the psychoanalysts regarding their ability to explore the unconscious. Such acceptance does not even carry acquiescence to any particular definition of the unconscious. As used by the present writers the terms 'subconscious' and 'unconscious' indicate merely that undetected and forgotten dispositions may play an important rôle in the determination of conduct.

Unconscious Conflict

In connection with the present discussion the fact should perhaps receive mention that many of the problems discussed by the psychoanalysts are discussed also by those who deny the concept of the unconscious as usually advanced. Lack of available terminology and clear-cut concepts are thus a handicap in the discussion of some very important psychological issues. For example, Freud maintains that the consciousness of civilized man is dominated by a 'censor' which prevents unpalatable experiences emerging from the subconscious. Watson, although he would disagree with Freud's terminology, explains censorship from a biological standpoint, maintaining that as children we take on certain habits which must be replaced later by other sets of habits. Often the replacement is not wholly successful. The phrase, "When I became a man, I put away childish things," represents an ideal rather than actuality (19). The biological basis of the unfulfilled wish is as follows:

We believe that one group of habits can 'down' another group of habits—or instincts. In this case our ordinary system of habits—those which we call expressive of our 'real selves'—inhibit or quench (keep inactive or partially inactive) those habits and instinctive tendencies which belong largely to the past (19).

Again Watson writes as follows:

I have for some years been an ardent student of Freud (and other psychoanalysts), but the further I go into their terminology the more sure I am that there is a simpler and more commonsense way (and at the same time a more scientific way) of describing the essential factors in their theory. I am convinced of the truth of Freud's work, but as I teach the Freudian movement to my classes I drop out the crude vitalistic and psychological terminology, and stick to what I believe to be the biological factors involved in his theories (Freud himself admits the possibility of these). The central truth that I think Freud has given us is that *youthful, outgrown, and partially discarded habit and instinctive systems of reaction can and possibly always do influence the functioning of our adult systems of reactions, and influence to a certain extent even the possibility of our forming the new habit systems which we must reasonably be expected to form* (20).

Watson points to the fact that in his discussion of the conflict which takes place between the various 'selves' William James long ago recognized the tendency of discarded habits to persist and effect one's later experience, particularly as it relates to further habit formation (20).

In an article entitled 'The Myth of the Unconscious,' Watson applies the phrase 'unverbalized behavior' to those acts which the psychoanalysts would describe as 'unconscious' (21). H. L. Hollingworth is another writer who prefers not to employ the term unconscious as it is ordinarily used. He asserts that abnormal behavior can be interpreted in terms of direct substitute reaction, the shifting of responses and subresponses by association being all that is necessary to account for most cases of abnormal behavior (22).

James Harvey Robinson takes cognizance of the unconscious, asserting that there is no special mystery about it. In his opinion the unconscious consists merely of the forgotten

and the habitual elements in our behavior (23). According to him the so-called unconscious conflicts are a fact; they give rise to numerous forms of substitute adjustment of which rationalization is a conspicuous and general example. Of rationalization he says:

> It has gone on in all the philosophers, scientists, poets, and theologians that have ever lived. Aristotle's most abstruse speculations were doubtless tempered by highly irrelevant reflections. He is reported to have had very thin legs and small eyes, for which he doubtless had to find excuses, and he was wont to indulge in very conspicuous dress and rings and was accustomed to arrange his hair carefully.[2] Diogenes the cynic exhibited the impudence of a touchy soul. His tub was his distinction. Tennyson in beginning his 'Maud' could not forget his chagrin over losing his patrimony years before as the result of an unhappy investment in the Patent Decorative Carving Company. These facts are not recalled here as a gratuitous disparagement of the truly great, but to insure a full realization of the tremendous competition which all really creative thought has to face, even in the minds of the most highly endowed mortals (23).

Harry Elmer Barnes believes that the great majority of human psychic reactions are produced not by instinctive or by habitual drives but by impulses or motives that are below the level of consciousness. He points out that the motives or reasons assigned by an individual for his particular act or policy, regardless of the honesty or the reliability of the individual, may be as far from the real truth as the statement of the most notorious liar of his generation (24). This is of course due to the fact that the individual's unconscious impulses do not permit introspective analysis.

However one may explain certain desires which may be unconscious or which may be simply 'denied' by the possessor because they are not compatible with society's demands, these subtle forces do appear to be important in determining the direction and the strength of an individual's reactions. Sometimes the resultant reactions are of conspicuous merit and social significance. For example, Barnes explains St. Au-

[2] Diogenes Laetius, book v.

gustine's religious zeal as a result of the latter's 'impurity complex.' The reader is of course aware of St. Augustine's tremendous influence upon the intellectual and social history of mankind. Barnes believes that St. Augustine's attitude toward sex impurity was wholly a psychic compensation for his wild youth and sexual excesses. Barnes advances also the hypothesis that Napoleon's immense drive and energy were due to the unusual dynamic force of his unconscious effected by epilepsy (**24**), pp. 364 *ff.*

Undoubtedly thwarting of desires results in much outstanding endeavor and consequent attainment. Kropotkin and Bakunin became advocates of liberal ideals as a reaction to the autocratic and militaristic Russian society. Numerous militaristic leaders apparently have developed their impelling purposes from thwarted desires.

> It is, further, common knowledge that the great individualists of the last century, Jefferson, Spencer, and Mill passed their childhood under an abnormally complete and severe domination and control by their male parent or some older male relative. . . . Our own history furnishes ample evidence of the pressing need for a reconsideration of the traits of our leading personalities in the light of the newer psychology . . . the violent anti-authority complex of Jefferson and its attendant feeling of inferiority, both of which were elaborated and defended in ten thick volumes of letters and public documents; the remarkable development of a 'spotless soul' in James Monroe after his part in many questionable episodes such as the violation of his word in the publication of the Reynolds letters; the sadism and the 'Jehovah complex' of Andrew Jackson, who was so intolerant of opposition as to be unable to complete a sentence in public debate without choking with anger (**24**), pp. 364–65.

The preceding discussion reveals a community of interest between such widely divergent writers as Freud, William James, J. B. Watson, H. L. Hollingworth, E. S. Robinson, J. H. Robinson, and Harry Elmer Barnes.[3] It is evident that

[3] Of interest in this connection is the announcement that at the Fourth International Conference of the New Educational Fellowship, held recently at Locarno, Switzerland (Aug. 3 to 15, 1927), psychoanalysis was approved as a subject for intensive study on the part of prospective teachers. See *School and Soc.*, 1927, **26**, pp. 228–29.

each touches upon the same fundamental problem, although each employs distinctive terminology. This lack of a common terminology is to some extent due to conceptual differences. However, it is clear that all four writers agree that mental conflicts may occur within an individual without occupying the focus of his attention.

Thus it is that the thwarted individual, particularly if he be hypersensitive, is driven unconsciously [4] to seek satisfying states of mind. Impelling motives thus arise. It is the belief of the present writers that when supported by a reasonable amount of ability and favorable opportunity, such drive may give rise to noteworthy achievement.

Most writers who stress the importance of the subconscious in modifying conduct stress also the significance of thwarted sex propensities. Indeed, one writer attributes most human maladjustment to unsuccessful attempts to gratify sexual desires. Not only do these writers trace human maladjustment to this cause; they cite also conspicuous attainment occasioned by the drive of this impulse.

Origin of the Æsthetic Impulse

Numerous writers have emphasized the rôle of sex in originating various forms of æsthetic impulses. That there exists a rather intimate relationship between the sex propensity and the appreciation of beauty, most careful students will grant. Santayana, for example, finds in the sex impulse, the origin and drive to the appreciation of beauty in its multitudinous forms.

> If any one were desirous to produce a being with a great susceptibility to beauty, he could not invent an instrument better designed for that object than sex. . . .
>
> What more could be needed to suffuse the world with the deepest meaning and beauty? The attention is fixed upon a well-defined object, and all the effects it produces in the mind are easily regarded as powers or qualities of that object. But these effects are here powerful and profound. The soul is stirred to its depths. Its hidden treasures are brought to the surface of consciousness. The

[4] As here employed the term 'unconsciously' posits merely that the source of his annoyance may be undetected or forgotten by the individual who is being driven.

imagination and the heart awake for the first time. All these new values crystallize about the objects then offered to the mind. If the fancy is occupied by the image of a single person, whose qualities have had the power of precipitating this revolution, all the values gather about that one image. The object becomes perfect, and we are said to be in love. If the stimulus does not appear as a definite image, the values evoked are dispersed over the whole world, and we are said to have become lovers of nature, and to have discovered the beauty and meaning of things. . . .

When love lacks its specific object, when it does not yet understand itself, or has been sacrificed to some other interest, we see the stifled fire bursting out in various directions. One is religious devotion, another is zealous philanthropy, a third is the fondling of pet animals, but not the least fortunate is the love of nature, and of art; for nature also is often a second mistress that consoles us for the loss of the first . . . (25).

According to Santayana, when love lacks a specific object, when it does not yet understand itself, or when it is sacrificed to other interests, the stifled energy is sublimated in various ways, susceptibility to the beauties of nature being one conspicuous form of sublimation. A similar point of view has been expressed by Langfeld who holds that æsthetic appreciation may result from the thwarting of *any* impulse or desire.

In the many and varied forms of art each individual can find his particular refuge. The strong and the weak, the rich and the poor, the prude and the libertine, the prince and the peasant, can one and all select that form of art in which they can best realize a harmonious adjustment of the conflicting impulses which tend to torture and distort the soul. . . . So long as the human race is stirred by conflicts just so long will art itself endure (26).

Buermeyer asserts that appreciation of art offers a more satisfying refuge from thwarting desires than the day-dream since objects of art afford tangible actualities to satisfy sublimated desires (27).

The above writers all agree that appreciation of art is to a large extent the outcome of sublimated desire. This hypothesis is in line with the present-day tendency to attribute the origin of much artistic creation to the attempt of the artist to

secure compensatory gratification for precluded desires. Thus the actuation of the work of art as well as the appreciation thereof may originate in the desire for substitute gratification.

Opportunity Also a Requisite to Effective Ability

It was previously pointed out that certain writers assume that genius 'will out.' They assert that inordinate capacity carries with it the ability to seek successfully the opportunity which will permit the ability to express itself. In the opinion of the present writers this assumption is a gratuitous one. No doubt everyone will admit that it is conceivable that a potential Napoleon might meet death in his first military encounter. The potential military genius so beset would be denied of course opportunity to manifest his capacity for military organization and command. It should be equally evident that every individual is to some extent a product of his age and that different ages afford diverse types of opportunity to the individual living therein.[5] A few examples will suffice to illustrate this fact.

In the present century it is possible for a few individuals to obtain renown by exercising their ability to play baseball. But this sport was not known in early times. It therefore seems plausible to assume that a few individuals of earlier ages may have possessed the capacity of a Babe Ruth for playing baseball yet, owing to the fact that opportunity was lacking, they were unable to develop their potential ability.

The case of Lindbergh is in point here. The airplane afforded Lindbergh opportunity to attain eminence. It is

[5] Terman has recognized the rôle of opportunity in the following: "A research now in progress on the mental development of 300 of the most eminent men and women of the last few centuries reveals the fact that many of these geniuses owed their greatest opportunities to happy chance. Newton could never have made his scientific and mathematical discoveries without a university education, and he owed his opportunity to enter Cambridge to the chance visit of an uncle. Liebig, the founder of physiological chemistry, was early withdrawn from school and apprenticed to a pharmacist. There, in a shed behind the apothecary's shop, he began the chemical experiments that later brought his chance to enter a university. One wonders whether those experiments in chemistry would have been undertaken had he been apprenticed to a shoemaker or tailor instead of to a man who dealt in chemicals. . . ." Terman, L. M., 'The Conservation of Talent,' *School and Soc.*, 1924, **19**, pp. 359–64.

extremely doubtful that Lindbergh would have attained equal eminence had he lived a century earlier.

Mr. Ford's achievement in the manufacture of automobiles is a conspicuous example of the importance of opportunity in effecting attainment. Had Ford lived in a civilization which had not discovered the use of iron, it is exceedingly doubtful that he would have attained equal eminence in any other field.

The preceding citations illustrate a rather obvious fact, namely, that environment must be propitious if the capable individual is to attain eminence. To assume that genius will out is almost equivalent to assuming that the individual is not dependent upon his predecessors, his co-workers, or other circumstances in which he chances to find himself.

In the light of the preceding discussion the present writers reiterate their conviction that ability is not identical with *effective* ability. It is their belief that the fruits of genius are a function of no less than three integers, namely, *ability*, *drive*, *and opportunity*. This position is to them unassailable and one not requiring elaboration but for the tendency on the part of some recent writers to confuse *ability* with *effective ability*, thus over-simplifying the problem of genius.

REFERENCES

1. Lehman, H. C. and Witty, P. A., 'Drive: A Neglected Trait in the Study of the Gifted,' Psychol. Rev., 1927, 34, pp. 364–76.
2. Galton, F. and Schuster, E., 'Noteworthy Families,' London, John Murray, Albermarle St., 1906, pp. xlii–96 (p. xix).
3. Woodworth, R. S., 'Dynamic Psychology,' New York, Columbia University Press, 1922, 210 pp. (p. 133).
4. James, W., 'The Will to Believe and Other Essays in Popular Philosophy.' ('Great Men and Their Environment,' pp. 216–255.) New York, Longmans, Green and Co., 1923, pp. xvii–332, (p. 242).
5. Seashore, C. E., 'Introduction to Psychology,' New York, The Macmillan Co., 1923, pp. xviii–427 (pp. 58 *f.*)
6. Robinson, E. S., 'A Concept of Compensatory Behavior and Its Psychological Setting,' *J. Abn. and Soc. Psychol.*, 1922–1923, 17, pp. 383–94 (p. 387).
7. Thorndike, E. L., 'Educational Psychology.' (Briefer Course) New York, Published by Teachers College, Columbia University, 1923, pp. xii–442 (pp. 4 *ff.*)
8. Wada, T., 'An Experimental Study of Hunger in its Relation to Activity,' *Arch. Psychol.*, No. 57, 1922.
9. McDougall, W., 'Social Psychology,' Boston, John W. Luce & Co., 1921, 418 pp. (pp. 42–44).
10. Watson, J. B., 'Behaviorism,' New York, The People's Institute Publishing Co., 1924, 251 pp. (p. 63).

11. WATSON, J. B., *op. cit.*, p. 65.
12. DREVER, J., 'An Introduction to the Psychology of Education,' London, Edward Arnold & Co., 1923, pp. viii–227 (pp. 8 *f.*)
13. LEHMAN, H. C. AND WITTY, P. A., 'Playing School: A Compensatory Mechanism,' PSYCHOL. REV., 1926, 33, pp. 480–85.
14. LEHMAN, H. C. AND WITTY, P. A., 'Church and Sunday School Attendance of Negro Children,' *Relig. Educ.*, Jan. 1927, pp. 50–56.
15. RAUP, R. B., 'Complacency: The Foundation of Human Behavior,' New York, The Macmillan Co., 1925, pp. xii–302.
16. THURSTONE, L. L., 'The Stimulus-Response Fallacy in Psychology,' PSYCHOL. REV., 1923, 30, pp. 354–69.
17. ROBINSON, E. S., *op. cit.*, p. 389.
18. MARKS, J., 'Drugs and Genius,' New York, Adelphi Co., 1925, pp. viii–185 (p. 77)
19. WATSON, J. B., 'The Psychology of Wish-Fulfillment,' *Scient. Mo.*, 1916, 3, pp. 479–87 (p. 483).
20. WATSON, J. B., 'Behavior and the Concept of Mental Disease,' *J. Phil., Psychol. and Scient. Meth.*, 1916, 13, pp. 589–97 (pp. 589 *f*).
21. WATSON, J. B., 'The Myth of the Unconscious,' *Harp. Mag.* Sept. 1927.
22. HOLLINGWORTH, H. L., 'The Psychology of Functional Neuroses,' New York, D. Appleton & Co., 1920, pp. xiii–259 (pp. 55 *ff*).
23. ROBINSON, J. H., 'The Mind in the Making,' New York, Harper & Brothers, 1921, 235 pp. (pp. 45–47).
24. BARNES, H. E., 'Psychology and History,' *Amer. J. Psychol.* 1919, 30, pp. 337–76 (p. 361).
25. SANTAYANA, G., 'The Sense of Beauty,' New York, Charles Scribner's Sons, 1898, pp. ix–275 (pp. 60 *f*).
26. LANGFELD, H. S., 'The Æsthetic Attitude,' New York, Harcourt, Brace and Howe, 1920, pp. xi–287 (pp. 280 *f*).
27. BUERMEYER, L., 'The Æsthetic Experience,' Merion, Pa. Published by the Barnes Foundation, 1924, 183 pp. (pp. 110 *f*.)
28. GATES, A. I., 'Psychology for Students of Education,' N. Y., Macmillan, 1923, pp. xvi–489. (pp. 125 *ff*.)

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A THEORY OF SIMULTANEOUS COLOR-CONTRAST.[1]

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The hypothesis which is the basis of this attempt to explain simultaneous color-contrast may be briefly summed up thus:

Unless spectral white light (light composed of all colors) *is mixed with the colored light from any surface, that surface will show no simultaneous color-contrast.*

This of course needs proof, which such experimenting as I have been able to do seems to give. I was unable to use the results obtained by former experimenters, as I could find no record of cases where care was taken to exclude spectral white light; indeed most experiments have been performed with colored papers, the pigments of which reflect much white light along with the color. In fact, Külpe says: "The more saturated the color, the less is it liable to apparent alteration by its surroundings." This goes to prove my hypothesis.

No observer, so far as I know, has ever seen contrast effects in the spectrum, although it displays bands of various colors ranged side by side and at different degrees of saturation. Blue induces no yellow on green, red no blue-green on yellow; this is a natural thing according to the hypothesis, for in the case of the spectrum, no white light can be present.

Still, in the case of the spectrum, conditions are not as favorable as possible since the reacting surface should be considerably smaller than the inducing one, and surrounded by it. I therefore decided to use colored glass in my experiment. Glass does transmit some white light, but in an extremely small quantity as compared with the white light reflected from colored pigments.[2] And if no contrast effect is

[1] This paper was submitted in partial fulfillment of the requirements for the degree of Master of Arts in the Faculty of Philosophy, Columbia University, in 1908.

[2] (This conclusion, which I reached independently, I have recently found confirmed in Professor Rood's 'Text-Book of Colors.')

perceived when a small quantity of white light is transmitted in addition to the colored light, still less would it be perceived if white light were totally excluded.

The apparatus for the experiment consisted of eight pieces of glass 6 x 3 in.—two each of red, yellow, green and blue—and a strip of each color 6 x ½ in. (The glass came from the C. H. Stoelting Co., Chicago.) One 6 in. edge of each large strip, and both 6 in. edges of each small strip, had been ground smooth, so that a small strip would fit accurately between two of the larger pieces. The slips of colored paper from a Milton Bradley color book were arranged in groups of five (one spectral color and its two shades and two tints), each group being pasted on a separate sheet of white paper. This was for the observer's convenience in matching. There was also a box lined with black velvet, having at one end an opening 4 x 1½ in. surrounded by an eyepiece like that of a stereoscope, made of stiff paper also lined with black velvet. At the opposite end of the box was an opening 4 x 4 in. covered by transparent paper. Grooved pieces of wood fastened against the outside of the box above and below the top and bottom of the opening served to hold the glass in place against the transparent paper.

The black lining and the eyepiece were intended to prevent the admission of light other than that which came through the square opening; for the same reason the edges of the glass were smoothed. The paper was designed to soften the lines of juncture and to give homogeneity of surface.

In performing the experiment, one of the glass strips was placed between two large pieces (green between blue or yellow, yellow between red or green, etc.) and all were slipped into the grooved pieces of wood, which held them before the square opening. To the observer, looking in at the other end of the box, there appeared a colored field 4 x 4 in. with a ½ in. band of another color passing perpendicularly through its middle. The observer was then asked to match the strip as nearly as possible from the Milton Bradley papers. The experiment was performed on two persons besides the writer, both women, one a college graduate of some years' standing,

the other an artist of considerable ability. Neither knew the results obtained by the other, nor the object of the experiment.

TABLE I

Blue Strip between			Subject	Yellow Strip between		
*White	Red	Green		White	Red	Green
Blue	Blue with a tendency to violet	Blue	*A*	Orange yellow	Orange yellow (paler)	Orange yellow
Blue	Violet blue	Blue	*B*	Orange yellow	Orange yellow	Orange yellow
Blue	Blue	Blue	*C*	Orange yellow	Orange yellow (lighter)	Orange yellow

*White means no glass.

TABLE II

Red Strip between			Subject	Green Strip between		
White	Blue	Yellow		White	Blue	Yellow
Orange red	Orange red	Orange red	*A*	Green (yellow in it)	Green (nearer yellow-green than before)	Green
Red	Red	Red	*B*	Green	Green	Green
Red	Red (a trifle paler)	Red	*C*	Green	Green (a little lighter)	Green

As the tabulated results show, in no case was color contrast perceived where white light was not also present; though brightness contrast was evident in several instances. Observer *A*'s 'orange red' and 'yellow in green' are accounted for by the fact that the walls and ceiling of the room where this experiment was performed were tinted yellow. The other experiments were performed in a different place. I do not know how to explain *A* and *B* both seeing blue in a red field as tending toward violet.

Assuming the hypothesis proved, an answer was then sought for this question: Supposing a color to be eliminated

from spectral white light, what color would the remaining rays produce? There is abundant evidence to show that it would be the color complementary to the eliminated one. The

TABLE III

	White Strip between					
	Red	Green	Subject	Blue	Yellow	
	No color	No color	*A*	No color (some people might see yellow)	Suggests blue if any color	
	No color	No color	*B*	Yellowish white	Bluish white	
	No color	Faint purplish pink	*C*	Very faint tinge of yellow	Faint bluish tinge	

A is the artist, *B* the college graduate, and *C* the writer.
* White means no glass.

phenomena of 'surface-color' illustrate this: certain substances (among them the blue, violet and green anilines, carmine and potassium permanganate) totally reflect light rays of certain wave-lengths, while they permit all others to pass. Kundt says (*Pogg. Ann.*, 142, p. 164): "The light transmitted through these substances is altogether or almost complementary to that reflected from the surface." This is what one would naturally expect. Stout says ('Manual of Psychology,' p. 156): "Every discernible color of the spectrum possesses its complement," and therefore spectral white light may be thought of as consisting of pairs of colors. Removal of one color leaves its complement alone, but weakened by the white light composed of all the other pairs. Hence the reacting surface is usually altered only *in the direction* of the color complementary to the inducing color.

Granted these premises, then, may not simultaneous color-contrast be due to the elimination from perception of a component of the white light reflected from the reacting

surface, this component being similar in color to the light from the inducing surface? The perceived color of a gray spot on (say) a blue field would then be gray tinged with yellow. In cases where the spot is colored, but at a low degree of saturation (as it must be, says Külpe, to show color contrast), the resultant color combines with the color of the spot; thus red on blue appears orange.

How and why does this elimination take place? The answer is found in the law of relativity. When a strong stimulus acts upon a sense-organ a similar but much weaker stimulus acting on the same sense-organ goes unperceived. So, when we see a gray spot on a green field, the green component of the light from the spot is swallowed up by the green of the field and fails of entrance to consciousness. So the spot looks 'pink.'

This explanation of simultaneous color-contrast accords with any theory of vision which postulates different retinal processes for different wave-lengths. It agrees with Helmholtz's explanation in being psychical, but does not make the perception of contrast an error of judgment. It might be translated into the language of Hering's color theory; but little would be gained, in my opinion, as his assimilation and dissimilation hypothesis seems to me most improbable.

I do not know of any case of simultaneous color contrast to which this explanation does not apply. It clears up some points which most color theories ignore. For instance, the magical efficiency of transparent white paper in producing contrast effects, as in Meyer's experiment, is due merely to its adding white light to that reflected from the gray or colored spot, at the same time softening contours and giving homogeneity of surface. Again, it explains why gray shows contrast better than white; there is less of the background color to be eliminated. And, as I have said before, it accounts for the fact that a gray reacting surface usually receives only a *tinge* of the color complementary to the field.

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